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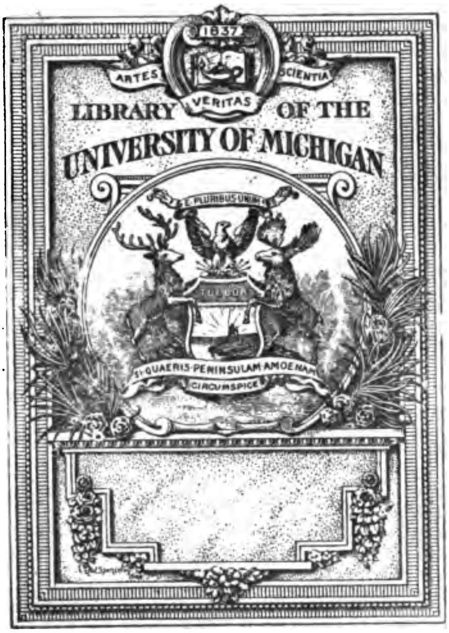
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THE MONTHLY
MICROSCOPICAL JOURNAL:

34808

TRANSACTIONS
OF THE
ROYAL MICROSCOPICAL SOCIETY,
AND
RECORD OF HISTOLOGICAL RESEARCH
AT HOME AND ABROAD.

EDITED BY
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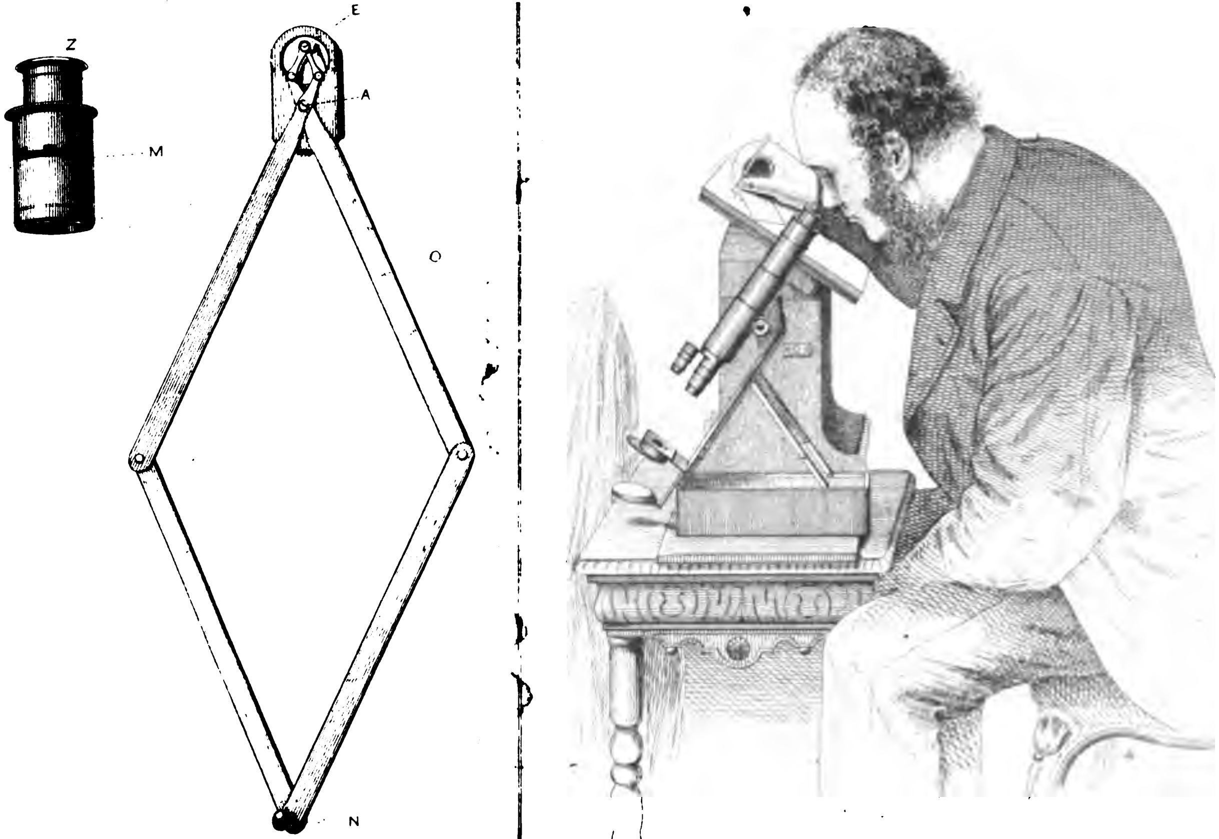

Mr Roberts' Pantograph

THE
MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1872.

I.—*On a Micro-pantograph.* By Isaac Roberts, F.G.S.

(*Taken as read before the* Royal Microscopical Society, *June* 5, 1872.)

Plate XXI.

Dr. Beale, F.R.S., in his treatise on 'How to Work with the Microscope,' says:—"It may be truly said that no real advance in our knowledge of the minute structure of animal or vegetable tissues can be communicated to others, unless accurate drawings are made." . . . "We may feel sure that those who follow us will respect our drawings, if honest copies of nature, although very little of what is now written will be read some years hence." These statements will be accepted as truisms, and yet how few are the observers with the microscope who can draw at all, not to say draw truthfully.

This defect is not to be wondered at, for the difficulties are

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EXPLANATION OF PLATE XXI.

Fig. 1.—The letters E, A, O, and N, indicate parts of the micro-pantograph. E is the glass disk, with micrometer cross-lines ruled upon it. It is cemented over a small hole drilled through the centre of the rivet which forms the joint at the minor extremity. A is the centre or fulcrum round which the instrument freely moves. O is one of the major limbs. N is the drawing-pencil holder. It is placed over a hole which is drilled through the rivet which forms the joint at the major end. Z is the eye-piece of the microscope. M is a slit cut into the side of the eye-piece, into which slit the minor end of the micro-pantograph and the support shown under E, A, Fig. 1, are inserted and firmly fixed. To draw any object it is only necessary to place the end of the pantograph in the slit of the eye-piece; place the eye-piece in the microscope; adjust the height and inclination of the drawing table and paper or pad to the plane of the pantograph, and with the right-hand forefinger and thumb guide the drawing pencil with a slight pressure over the paper, at the same time looking through the eye-piece at the object and guiding the centre of the micrometer cross-lines over the respective parts of it, and an accurate drawing of the object will be traced upon the paper. I obtained a very satisfactory result even with the first trial, the object drawn being the leg of a bee.

Fig. 2 is a general view of the drawing arrangements when they are in use. The proportions of an instrument made to draw the apparent size of magnified objects are as follows:—Length of the minor sides of the parallelogram within the eye-piece, half an inch; length of the major sides, 5¾ inches. The micro-pantograph when extended the full length measures 12¼ inches.

great. I cannot decide whether it is more difficult to draw with the aid of the appliances hitherto in use, or by the eye without them.

I now introduce to your notice a very simple instrument which I have made, and which I think will enable any person, after a few hours' practice with it, to draw with great ease and accuracy. I call the instrument a *Micro-pantograph*, and the plate which accompanies this paper, and following description, will enable you to understand its construction and mode of application to the microscope for the purpose of drawing.

I do not patent the instrument, but freely present it to the workers engaged in the cultivation and advancement of microscopical science. Any microscopist who possesses a little skill in mechanical operations may easily make a micro-pantograph for himself; or any optician can make it and adapt it to any microscope at a trifling cost.

Following is a description of the instrument:—It consists essentially of two parallelograms having their major and minor sides and angles respectively proportioned in all positions in which the instrument can be placed. The major and minor sides rotate freely about the centre, or fulcrum, which they have in common, and which is fixed to the eye-piece of the microscope, in the focus of the eye-lens. A pencil is attached to the major end joint of the instrument, and a small glass disk, ruled with a micrometer lined cross, is attached to the minor or eye-piece end joint, in the position where pointers are placed; and similar focussing of the eye-lens is requisite to see both the cross and the object at the same time that is necessary to see a pointer and object. When drawing, the hand guides the pencil which marks the drawing upon the paper, and at the same time guides the micrometer cross-lines over the field where the object appears in the microscope.

The drawing-paper, or pad, should be attached to a small table (shown in Fig. 2), which is capable of being adjusted to the required height for the microscope used, and the top must be made to incline to any required angle, according to the position of the microscope.

II.—*On Bog Mosses.* By R. BRAITHWAITE, M.D., F.L.S.

3. Sph. rubellum Wilson.

Bryol. Brit. p. 19 (1855).

PLATE XXII.

Syn.—*Sph. rubellum* Wils. l. c. T. LX. Schimper, Torfm. p. 70, T. 20 (1858); Synop. p. 680 (1860). Berk. Handbk. p. 306 (1863). Lindb. Torfmos. No. 12 (1862). Hartm. Skan. Fl. 9th ed. II, p. 83 (1864). Milde, Bryol. Siles. p. 383 (1869).—*Sph. acutifolium*, Var. ζ *rubellum* Russow, Torfm. p. 41 (1865).

Dioicous. Plants slender, laxly tufted, resembling the small flaccid red form of *S. acutifolium* in habit, and *S. tenellum* in tenuity. Stem fragile, 2–5 in. high, sublignose, purple, coated with a thickish bark composed of *four strata of cells, destitute of pores.* Ramuli in distant fascicles, shortish, often curved or twisted, three, 2 arcuate, 1 deflexed; their flask-shaped cells with the apex prominent, and a little recurved. *Cauline leaves large,* erect or reflexed, *broadly ovate-oblong, concave,* finally plicate, furnished at extreme apex with several small teeth, *narrowly margined,* hyaline cells large 2–3 partite, non-fibrose, non-porose. Ramuline leaves at base of branch very small, ovate, at middle *oblong-ovate, with the margins inflexed in the upper part, elliptic when expanded under pressure, gradually acuminate, the point with three stoutish teeth,* and several minute ones below them; leaves of deflexed branches elongated, all narrowly margined, reddish, here and there yellow or green; *hyaline cells with annular fibres,* sparingly porose, gradually smaller toward the edge, strongly projecting and confluent at the back, on the inner side removed from each other by the interposed chlorophyll cells.

Male plants like the female, and more numerous; amentula short, clavate, purple with broadly ovate bracts.

Fruit immersed in the capitulum or on a shortish peduncle; peduncular leaves numerous, loosely imbricated, subrecurvo-patent

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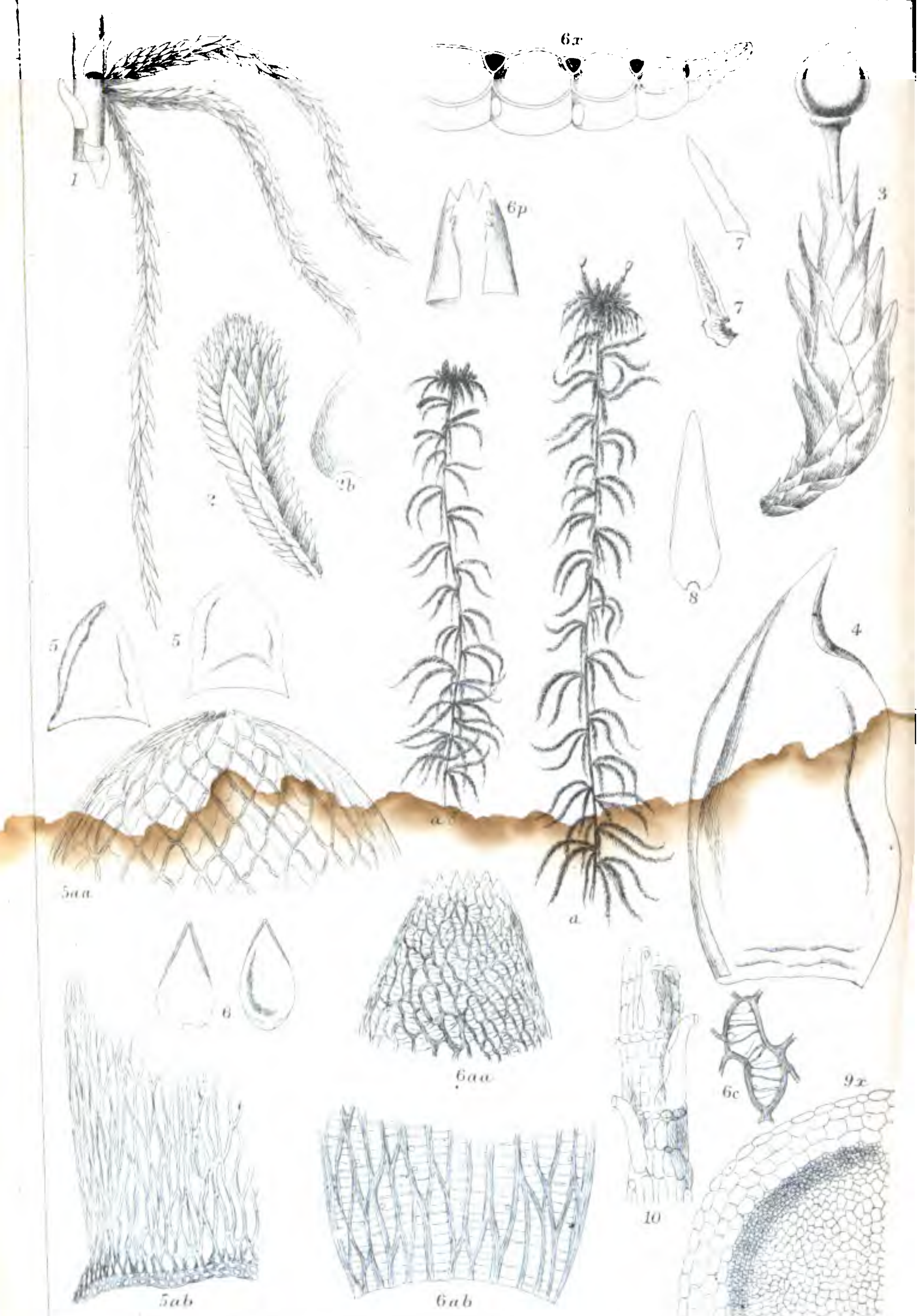
EXPLANATION OF PLATE XXII.

Sphagnum rubellum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b*.—Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a*.—Areolation of apex of same. 5 *a b*.—Ditto of base.
6.—Leaves from middle of a divergent branch. 6 *x*.—Transverse section. 6 *p*.—Point of same. 6 *a a*.—Areolation of apex. 6 *a b*.—Areolation of base. 6 *c*.—Single cell from middle × 200.
7.—Intermediate leaves from base of a divergent branch.
8.—Leaf from a pendent branch.
9 *x*.—Part of section of stem.
10.—Branch denuded of leaves.

toward apex, elongate-oblong, slightly acuminate, convolute, straw-coloured. Capsule globose dark brown. Spores ochreous.

Hab. In bogs among grass and carices, often with *Sph. acutifolium* and *tenellum*. Fruit rare. June and July. Scandinavia, the Tyrol, England, Scotland, and Ireland. The specimen figured is from Risley Moss, near Warrington, sent by the late Mr. Wilson. It is probable that this species is often passed over as *Sph. acutifolium*, var. *purpureum*, to which it must be confessed it bears a very suspicious resemblance; and if we overlook the dioicous position of the inflorescence, there is no other character that will absolutely separate them. Russow indeed boldly unites them, and states, moreover, that *S. acutifolium* is sometimes dioicous: this I am unable to confirm; and as we find the reproductive organs in all mosses very constant in position, we can only refer it to *acutifolium* from a Darwinian point of view by supposing that the male and female organs after a long struggle have succeeded in maintaining separate establishments. The other points of distinction from *acutifolium* are, "the larger non-fibrose stem leaves and the broader branch leaves;" but I have shown that the presence or absence of threads in the hyaline cells may largely depend on conditions of growth, and is liable to variation in all species; the greater width of the leaves must be admitted, but this cannot be regarded as more than a relative character. Its habit appears to be constantly more slender than *S. acutifolium*, and the leaves of the divergent branches have the cells in their upper half differing less in size from the lower than they do in *S. acutifolium*, the threads in *rubellum* being also entirely annular; the pores are smaller than in *acutifolium*, and more equal in size at the base and apex of the leaf, and lastly, the catkins of male flowers are cuspidate in *S. acutifolium*, clavate and obtuse in *S. rubellum*.



R. Braithwaite del. ad nat.

West Co.

Sph. rubellum

III.—*Remarks on the Homological Position of the Members constituting the* THECATED SECTION *of the* CLASS ROTATORIA. By Charles Cubitt, F.R.M.S.

(*Taken as read before the* Royal Microscopical Society, *June* 5, 1872.)

Plates XXIII. and XXIV.

By Thecated I mean all those animals that are invested in a Theca or sheath, which, whether organically attached to the animals or not, forms no part of the general integument of the body which will be denominated the *corinum.* These Forms, moreover, are all supported on a *pedicle,* or footstalk, by means of which they attach themselves permanently to some support or basis, either individually or in clusters.

In a somewhat hurried epitome communicated to the 'Monthly Microscopical Journal' in October last, I confined myself to a cursory review of the homologies of two species, the first, a Form that I had inadvertently placed in the Genus Floscularia as Floscularia *coronetta;* and the second, Limnias *annulatus,* a rare species, which I in that communication called Melicerta *annulatus;* and finding that certain objections have been raised to the temerity of associating Melicerta, Limnias, Tubicolaria, and Œcistes in one Genus; Lacinularia, Megalotrocha, and Conochilus in another Genus; and placing these two Genera in the same Family while constituting a Family for the reception of Stephanoceros and Floscularia; I feel it incumbent on me to extend these remarks somewhat further in support of the views I had so briefly set forth; for having since that date been closely occupied in the collection of materials for a Memoir of the THECATED SECTION of the CLASS ROTATORIA, which I trust at no distant period to submit to the review of my brother microscopists, I have had ample opportunities for confirming the views I had so briefly set forth; and in this short review it will be necessary to confine my remarks to a consideration of their Dermal and Alimentary Systems.

I find myself following the steps of Mr. Gosse, who in the year 1851 published a consistent classification for the CLASS ROTATORIA in the 'Annals and Magazine of Natural History,' in which he subdivided these particular Forms which I define by the term THECATED, precisely as I find them work out on a careful consideration of their anatomical details. Mr. Henry Davis was fully aware of this when he described and so carefully delineated two new species, although he felt dissatisfied with the position in which he provisionally placed them; still, accepting the Ehrenbergian notion, the one which he denominated Œcistes *intermedius* should have been Limnias *intermedius.*

This SECTION divides itself into two Families—Floscularіæ, containing the two Genera Stephanoceros and Floscularia; and MELICERTADÆ, containing the two Genera Melicerta and Lacinularia: and to see at a glance where the first FAMILY differs from the second, and where the Genera of each agree in their general organization, the diagrams (Plate XXIII., Figs. 1, 2, 3) are submitted, on each of which a line is drawn from the mouth to the anus; and if we consider this to be the axis of the body, we find in the first FAMILY that both the marginal wreath of setæ, the *trochus* (which I employ also for the ciliated wreath of the second), and the secondary belt of cilia, the *cingulum*, surround the axis, and the anus is placed on the *neural* side, the side on which the ganglion is seated; but in the second FAMILY the trochus subtends the axis, and the cingulum, after traversing the margin of the *corona* (the miscalled *disk*), also surrounds the axis, while the anus is situated on the opposite, the *hæmal* side. The trochus is indicated on the figures by a full black line, and the cingulum by double fine lines.

There is a particular reason for rejecting such inconsistent terms as *dorsal* and *ventral* in this SECTION, for in the FAMILY FLOSCULARIÆ they virtually represent the *same* side of the animal. I shall therefore, with the examples of both Huxley and Allman before me, employ *hæmal* and *neural*, on the same grounds that Allman adopts them in his 'Memoir of the Fresh-water Polyzoa,' *viz.* that "notwithstanding an apparent contradiction in denominating as 'hæmal' any portion of an animal totally deprived of a blood-vascular system, they have the advantage of stating a simple fact."

The organization of these two FAMILIES differs conspicuously in their *physique*, and equally so in their DERMAL and ALIMENTARY SYSTEMS; and in selecting an individual from each, for the purpose of eliminating their true relations, I shall employ Floscularia *campanulata* to illustrate the one, and Melicerta *pilula* the other, from the fact that they have recently been recorded as 'New Species of Rotatoria' by Dr. F. Collins;* this last species being a Form with which I have for some years been intimately acquainted, which I had denominated Melicerta *pilula* (long before I committed my observations to print), from the fact that she fortifies the gelatinous basis of the theca with her own *excremental pilules.* This, as a new species, was recorded in a paper communicated to the Quekett Club in the year 1868 by Mr. J. G. Tatem, who at that date admitted that he had no specific title wherewith to denominate it. And Dr. Collins, in describing this, states that "the pellet with which the animal builds its tube is formed in a *kind of sac* situated at the lower extremity of the abdomen, and is discharged through the cloaca." He describes also a new species of Floscularia, which he denominates Floscularia *trilobata*, somewhat similar in appear-

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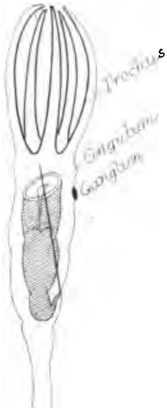
* 'Science Gossip,' January, 1872.

FLOSCULARIÆ

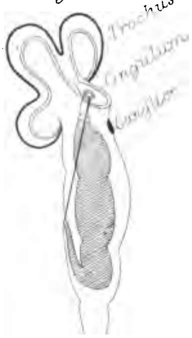
Fig 1.



Stephanoceros

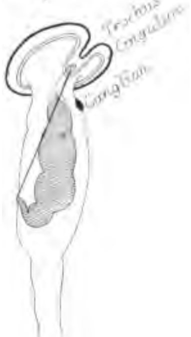
MELICERTADÆ

Fig. 2.



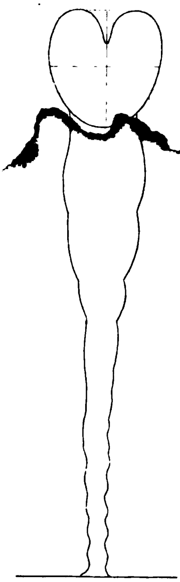
Melicerta

Fig. 3.



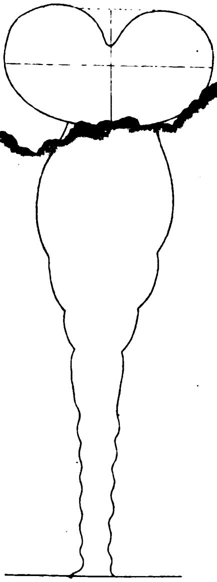
Lacinularia

Fig. 4.



Lacinularia socialis

Fig. 5.



Lacinularia albo-flavicans

C Cubitt del West & C° imp

Diagrams.

ance to F. *campanulata.* "The dorsal lobe is frequently much longer than the other *two,* and when such is the case it is *commonly* curved forwards over the funnel-shaped mouth, and presents a somewhat hooked appearance: the setæ are also *differently arranged,*" *i.e.* from those of F. *campanulata,* "being placed between the lobes as well as on their summits." "The cloaca is situated high in the body;" he shows it at the neck. Let Dr. Collins imagine himself in the same predicament, and he will have a case equally consistent with the laws of animal physiology. The so-called intestine is shown to extend all along the hæmal side of the body, on the side *opposite* to the ganglion; but in every individual of this FAMILY the anus is situated on the *same* side as the ganglion.

DERMAL SYSTEM.

The theca in the first FAMILY (Plate XXIV., Fig. 1) is secreted from a secerning gland situated at the foot, and the integument, after expanding at the base upon its selected habitat, ascends to the neck of the animal, and from thence it returns downwards along the body, forming an *annular* space between the inner and outer integuments, which space is filled with a hyaline fluid similarly with the *ectocyst* of Lophopus *crystallinus* of the Polyzoa. In Floscularia, however, this fluid becomes somewhat turbid in the older animals.

The inner lining forms a *tubular* space commencing at a point somewhat below the anus, thus leaving a free passage for the discharge of the ova and fæces along the body of the animal. The attachment of the theca with the pedicle differs in extent in different individuals. I have had ample opportunities of proving beyond dispute that this organic attachment of the theca is real, having liberated numerous patients from their selected supports without in any degree injuring the theca; and when in this free condition, and left with perfect freedom of motion, these animals made considerable invaginations of the theca with every regression of the body, which under such abnormal conditions were frequently repeated, both at their anterior and posterior regions.

The animals of the Genus Floscularia manifest a predilection for liberating themselves from their investing thecæ, and re-establishing themselves in new ones, a habit which is not practised by Stephanoceros. But there is a peculiarity with the theca of Stephanoceros that does not obtain to such a definite extent with any of the Floscularia; I refer to the corrugations which occur along the whole length of the theca, and upon the origin of these corrugations much misapprehension has been manifested. They have actually been attributed to the "tube sinking from its own weight"; the absurdity of which is at once apparent when we know that the specific gravity of the substance of the tube is *less* than that of the water in which, when liberated, they float. These corrugations are due to natural causes, aided by mechanical effects; they are produced by the acts of the animal's regressions, which are very frequently indulged in for periods of some hours' duration, and which are no doubt periods of repose, when beyond the mechanical effect of such a contracted position there is possibly a partial arrest in the secretion of the substance of the theca.

The theca of the FAMILY MELICERTADÆ, on the other hand, is never organically attached after it is once deposited; the gelatinous basis of which this foundation is formed is secreted by a secerning gland, situated in the anterior region, and in a neural aspect, in close proximity with the ganglion, and it is discharged from an orifice in the centre of the *figurum* (Plate XXIV., Figs. 2, 3, *r*). This cup-like organ is present in every individual of this FAMILY, and is furnished with cilia in every species that attaches any foreign substance to the theca. It is, however, unciliated in Melicerta *annulatus*, where the theca is free from any extraneous accession except such as become attached parasitically. In M. *pilula* it is also unciliated, for here the animal fortifies the gelatinous basis with her own voided excremental pilules. And in Lacinularia *volvox*, that free roving cluster, it is also unciliated where the theca is always devoid of any foreign accession.

There are two species in which the theca is constructed upon a symmetrical plan, Melicerta *ringens* and M. *annulatus*, and it attains a high degree of perfection in M. *ringens;* and I take this opportunity of recording that it has been my privilege to discover the particular organs by whose functions the mathematical precision with which the pellets are deposited upon the theca is governed. They have their homologue in M. *annulatus*, the only other species that constructs its theca on a symmetrical plan. I refer to the two " hooked spines" situated on the hæmal side of the neck, which are the most conspicuous organs on the emergence of the animal, and these have been erroneously called "lips." I must, however, content myself with stating that, notwithstanding their being situated on the hæmal side, they are employed in governing the mathematical precision with which the pellets are deposited on the opposite side; and in this brief review I confine myself simply to recording the fact. A more detailed description will be given in the 'Memoir' on which I am at present employed.

In the particular instances of Melicerta *ceratophylli*, M. *crystallinus*, and M. *intermedius*, the theca is formed of a consistent shape, and with well-defined walls, leaving a clear space for the motions of the inhabitants, but without any system of parts symmetrically arranged, and with these, the *upper* portions are more densely blended with foreign accessions than the *lower;* while with M.

The Monthly Microscopical Journal July 1. 1872. Pl. XXIV

Fig. 4.

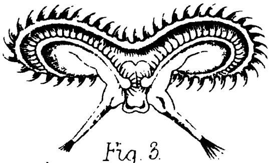


Fig. 1

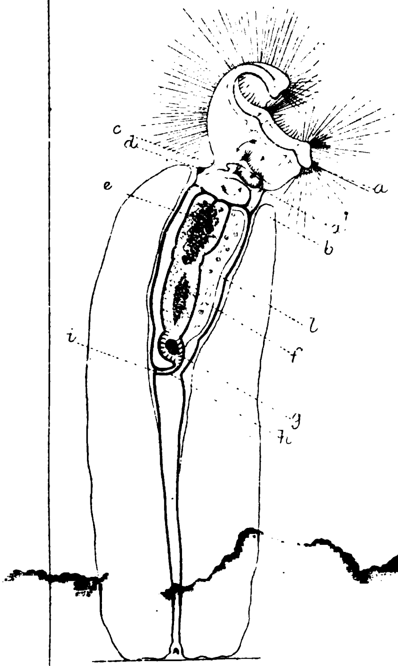


Fig. 3.

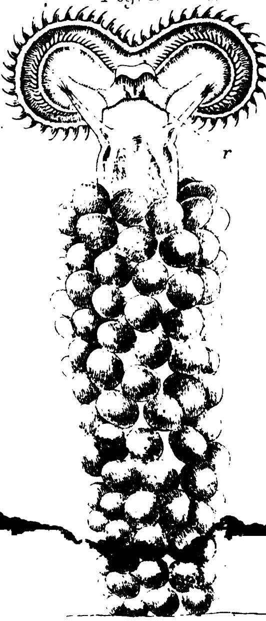
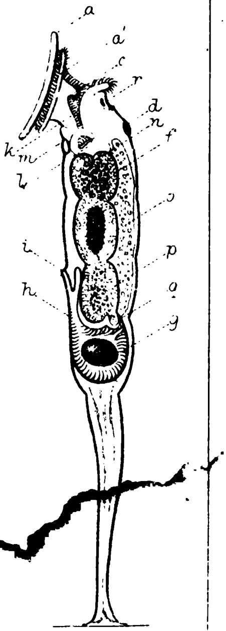


Fig. 2.



Floscularia Campanulata

Melicerta pilula

REFERENCE.

a. *Trochus*
a'. *Cingulum*
b. *Calix*
c. *Lingula*
d. *Ganglion*
e. [illegible]

f. *Stomach*
g. *Intestine*
h. *Rectum*
i. *Anus*
k. *Pharynx*
l. *Maxillæ*

m. *Parotid Gland*
n. *Cardia*
o. *Liver*
p. *Pancreas*
q. *Pylorus*
r. [illegible]

C. Cubitt del. West & Co.

Floscularia and Melicerta.

Najas and M. *longicornis*, the gelatinous basis is deposited in great profusion round about the animal, forming a corpulent mass apparently with no internal integument. But there is evidently some connection with the animal and the internal walls of the theca, for rapid regressions of the animal always produce an inversion of the substance of the theca at the top, but not a coalescence of the material and perfect closing of the top, as Mr. Davis assumes to be the case with M. *longicornis;* and in these two species the lower portions are more densely blended with extraneous matters than the upper. The substance of the theca of M. *Najas* is of a very supple consistency, for it frequently happens that diatomaceæ are involved within it, when they are seen to progress and regress in their usual manner, although with more difficulty. These two species manifest a similitude with the members of the Genus Lacinularia in the nature and deposition of the gelatinous basis of the theca.

In the Genus Lacinularia I place Megalotrocha and Conochilus, retaining, however, their specific distinctions for facility of recognition, as Lacinularia *socialis*, L. *albo-flavicans* and L. *volvox.* They all arrange themselves in clusters by an attachment of the foot either to some vegetable substance or otherwise to each other, as in the last instance, L. *volvox;* and notwithstanding the evidence that exists to the contrary, I beg to submit that the theca of L. *albo-flavicans* also becomes blended with foreign particles, similarly with the theca of L. *socialis*, when the animals attain a certain age; it has, as a natural consequence, the ciliated figurum, the same as L. *socialis;* while in L. *volvox*, where there is a total absence of foreign accessions, the figurum is unciliated.

I am fully prepared to find the allegation that I here record of an investing theca in L. *albo-flavicans* disputed, and I can only attribute the misconception that has hitherto prevailed on this matter, to the supposition that when a theca has been detected it has been accepted as L. *socialis.* A little incident that I here relate will confirm such to be possible. In the early part of last season Mr. Thomas Bolton, of Stourbridge, advertised for the benefit of microscopists a supply of Megalotrocha *albo-flavicans*, and being somewhat scarce in the neighbourhoods with which I am most familiar, I wrote for a supply, and was informed that "as they had all invested themselves in a gelatinous sheath, they must be Lacinularia." Mr. Bolton subsequently forwarded me a hand-sketch of an individual that he had "measured up and dimensioned on my system," and an inspection of this showed me at once that it was a *bona-fide* Megalotrocha, according to Ehrenberg's classification, and not Lacinularia, as my friend had been led to assume from the presence of the investing theca.

The points which distinguish the one species from the other are very conspicuous, which a glance at Plate XXIII., Figs. 4 and 5, will at once make manifest; for although they both exhibit a palpable bilaterality in the corona, they differ essentially in their respective characters, for we see in L. *socialis* that both axes of the disk are equal, while in L. *albo-flavicans* the horizontal axis is just double that of the vertical. Dujardin denies the propriety of their separation, but Ehrenberg considers the *absence* of a divesting theca, and the attachment of the ova by a filament to the body, to be sufficient grounds for separation. Now the very assumption of the filament attaching the ova to the body is the best evidence we could desire, to show that the group is invested in a gelatinous matrix. The *appearance* of the filament is simply the microscopical rendering of the integument of the theca itself surrounding the ova. Ehrenberg's drawing speaks for itself.

ALIMENTARY SYSTEM.

However dissimilar the organization of the setiferous trochus (*a*) of the one FAMILY may be with the ciliated trochus of the other, they are constituted to perform the same functions in the animal's economy as the first instrument employed in securing and directing prey to the mouth. In Stephanoceros *Eichhornii* and S. *Horatii* the corona is invested in one continuous series of setæ all along the margin of the trochus, but in Floscularia the lobes only are furnished with these prehensile organs, with the one exception of Floscularia *campanulata*, where they occur also in one continuous and unbroken series. Plate XXIV., Fig. 1.

In the first FAMILY the alimentary particles, when captured by the trochal setæ, are directed by their intermittent action into the *calix* (*b*), in which the ciliated *cingulum* (*a′*) is situated. I employ the term calix to the funnel that precedes the alimentary canal, not in its botanical significance of the leaf-like organs that surround and support the corolla, as Professor Allman has applied it to the diaphanous membrane surrounding the lower portions of the tentacles of the Polyzoa, but in its more general signification of a *cup*. The cingulum creates a vortex within the calix, in which the particles, alimentary or otherwise, become involved, passing by its influence upwards on the hæmal side, and downwards on the opposite, where they become subjected to the scrutiny of the tongue-like little organ the *lingula* (*c*), that exercises such a marvellous faculty in selecting from the heterogeneous mass those portions which are specially qualified for alimentary purposes, and in rejecting others as refuse; and situated on the neural side just above the ganglion (*d*), within the calix, its position and functions are identical with the *epistome* of the Polyzoa.

The calix communicates, by means of an œsophagus, with a capacious chamber, the *cardiacus* (*e*), and it is seen that alimentary matters, whether animal or vegetable, frequently pass through the *maxillæ* (*l*), suffering but little disintegration from their action when they return again and again into this chamber, the walls of which are lined with follicles, that, notwithstanding the position this chamber occupies in relation with the jaws, I assume to be *gastric* follicles, for the *stomach* (*f*) which succeeds it in this FAMILY consists of a single chamber, whose sides are thickened by large turgid cells that secrete the *bile*. The stomach discharges its fæcal contents at once into the *intestine* (*g*), wherein they become formed by the action of its cilia into crude pellets, that are discharged by a short rectum (*h*) to the anus (*i*), and the anus in this FAMILY is situated on the neural side, the same as with the Polyzoa, and is placed at the lowest extremity of the body, and not, as Dr. Collins represents it, on the hæmal side at the *neck* of his pseudo-new species: he has simply mistaken the integument of the ovarium, which *is* placed on the side opposite to that occupied by the anus, and it is frequently found to be devoid of ovarian nuclei. The rectum and anus are always placed on the neural side in this FAMILY, and at the lowest extremity of the body.

In the second FAMILY the ALIMENTARY SYSTEM differs essentially from the other, for the provender which is first brought by the persistent action of the trochal cilia under the influence of the ciliated cingulum, whose action creates two currents that act in opposite directions, the one from the right and the other from the left, by which the heterogeneous mass of particles is conveyed to the lingula (*c*), that is here situated on the opposite, the hæmal side, where, by the influence of this organ, the particles so involved are segregated and severally disposed both for alimentary and fabricating purposes; when those selected for aliment pass at once by a ciliated *pharynx* (*k*) to the *maxillæ* (*l*), which organ in this FAMILY is provided with a *parotid gland* (*m*), situated in close proximity therewith, and the maxillæ produce a much more complete degree of mastication than that organ in the first FAMILY. In MELICERTADÆ it occupies a bulbous chamber, which is separated from the *stomach* (*f*) by a *cardia* (*n*), and the stomach virtually consists of three chambers, for wherever digestion is concerned, certain ingredients are necessary for its due performance;—gastric, biliary, and pancreatic fluids—which in the vertebrates are secreted from a separate set of organs that communicate with the alimentary canal by ducts; but with the Rotifera the alimentary canal consists of a single channel divided into three compartments, that are each organized to secrete these several fluids, so that in Melicerta the first chamber is the stomach proper (*f*), whose sides are lined with *gastric follicles*, that convert the masticated food into *chyle;* the second (*o*) being the biliary chamber acting in the capacity of a *liver;* and the third (*p*) a *pancreatic* chamber wherein the *chyle* becomes converted into *chyme*, and from this chamber the digested food passes by means of a contractile *pylorus* (*q*) into the *intestine* (*g*), which is highly ciliated, and wherein the fæces become formed, by the rotation of the contained cilia, into well-defined pellets, that are ejected by a muscular contraction of the long *rectum* (*h*) to the anus (*i*), which acts as the common duct for both the fæces and the ova; and the monocyst at its junction with the anus is seen to be considerably folded in upon itself. In the particular instance of M. *pilula* the fæcal pilules attain a much greater bulk than in any other species, and with such pilules this little Melicertian fortifies the gelatinous basis of the theca, which, like every Form, is secreted from the figurum (*r*), that in this particular species is unciliated.

IV.—*Further Remarks on Tolles' $\frac{1}{8}$th and Powell and Lealand's Immersion $\frac{1}{16}$th.*

By Edwin Bicknell, Cambridge, Mass.

On page 225, vol. vi., of the 'Monthly Microscopical Journal,' I made some "Remarks" on Dr. Woodward's "Note" in the number for September preceding. In the number for January, 1872, vol. vii., page 28, Dr. Woodward says that I have "wilfully" misinterpreted his *published measurements* of the wet front of the Powell and Lealand $\frac{1}{16}$th. I have *not* misinterpreted the *actual power* of the above-mentioned objective, although Dr. Woodward's published measurements may look so exact—on paper; and in order to make out my case I shall not go beyond Dr. Woodward's own statements and "published" work, except in one case, to obtain my evidence.

On page 29, vol. vii., Dr. Woodward gives a table of measurements of different objectives from Powell and Lealand, both wet and dry: I wish to state here that I have nothing to say in regard to any of the *dry fronts*, and only with regard to *one* immersion $\frac{1}{16}$th, and that one is No. 2. I assume this to be the one from the magnifying power as given in the table agreeing with Dr. W.'s *published* measurements.

Dr. Woodward quotes from Mr. Charles Stodder* that "objectives are named when adjusted for uncovered objects, a fact not generally known by purchasers. The power increases, that is, the focus is shorter as the collar is turned to work through the covering glass." This is the method which I have used during the past two years, and I consider it the most certain and reliable.

During the past winter Mr. Stodder has loaned me the glass positives of *Amphipleura pellucida*, which were made at the Army Medical Museum, with the Tolles' $\frac{1}{8}$th described by Dr. W. in his "Note," and also the positive made with the Powell and Lealand $\frac{1}{16}$th at the same time. These positives are marked precisely like those described by Dr. W. in his "Note," and are probably duplicates of those sent to the Editor of this Journal by Dr. Woodward. I have carefully and repeatedly counted the lines in the positive made by the Tolles' $\frac{1}{8}$th (without eye-piece at 48 inches distance from the object to the screen). The length counted is exactly $\frac{1}{5}$th of an inch (the reason of this will be seen farther on), and in this space I find just 74 (seventy-four) lines, making 370 lines to the inch in the positive. Dr. Woodward gives the fineness of the lines on the *Am. pellucida* at 95,000 to the inch. Dividing 95,000 by 370 gives 256+ diameters as the power of the objective at 48 inches distance, which is just what Dr. W. has marked the positive. 48 divided by 256 gives ·1875, making the objective at that position of

* 'M. M. J.,' Oct. 1871, p. 203, note.

cover adjustment a little less than $\frac{1}{5}$th of an inch in equivalent focal length, that is, of higher power.

Now, on counting precisely the *same* portion of the *same* frustule in the positive made with the Powell and Lealand objective (the portion here counted being one *inch* long, whereas in the other it was one-*fifth*, but in both cases exactly the same portion), I find just 75 lines (seventy-five lines) to the inch. 95,000 divided by 75 gives 1266+ diameters at 48 inches distance. 48 divided by 1266 gives ·0387+, making the objective at this point of the cover adjustment a little less than a $\frac{1}{25}$*th of an inch in equivalent focal length.* It will be seen that this differs materially from Dr. Woodward's "published measurements." He gives in his table on page 29, vol. vii., 1100 diameters at full correction for thickest cover at 48 inches distance (yet on the glass positive which I have counted he has marked 1140 *diam*). I make it 1266+ diam. Let us reverse the process, take the Doctor's "published measurements," 1100 diam., and multiply it by 75, the number of lines to the inch in the positive; this will give 82,500 as the fineness of the lines on the *Am. pellucida* which he has photographed (1140 diam. will give 85,500), quite a falling off from his 95,000. If the "published measurements" are right, the *Am. pellucida* is not 95,000 to the inch.

It will be seen that instead of being about three times the power of the Tolles' $\frac{1}{5}$th, which Dr. W. has called a little higher than a $\frac{1}{5}$th—and my count of the photograph agrees closely with his statement—the Powell and Lealand objective is *five times* (very nearly) *the power.* This increase of fifty per cent. over the nominal power cannot be altogether ascribed to the cover adjustment, as Dr. W. in his table above quoted gives the entire range at less than twenty per cent. In fact, the objective is at least a $\frac{1}{19}$th at its uncovered point, and this *was known to Dr. Woodward* before he made the comparison with the Tolles' $\frac{1}{5}$th, with the disparaging assertion that "the new $\frac{1}{5}$th cannot be claimed to supersede the highest powers now in use, yet nevertheless is not, in my opinion, injurious to the $\frac{1}{5}$th." Mr. Charles Stodder writes, with good authority, probably under the date July 20th, 1871 (this was nearly two months before I saw Dr. Woodward's "Note"), "He has now ascertained that the Powell and Lealand so-called $\frac{1}{8}$th objectives are really $\frac{1}{10}$th; the so-called $\frac{1}{16}$th is a $\frac{1}{19}$th."* This did not reach this country until after my "Remarks" had been sent to this Journal, and I knew nothing of Mr. Stodder's communication until I saw it in print, and he knew nothing of mine until he saw it in print.

In the number of this Journal for April, 1872, page 166, Dr. Woodward alludes to a so-called $\frac{1}{30}$th by Wm. Wales, and after, "a determination of its magnifying power, however, shows that uncovered it magnifies rather less, at covered rather more, than the Powell and Lealand $\frac{1}{16}$th." Further, "its equivalent focal length at uncovered is therefore $\frac{1}{18}$th, very nearly." The same is repeated in the 'American Naturalist' for April, 1872. Why does Dr. Woodward still persist in calling an objective a $\frac{1}{16}$th, which he has repeatedly admitted to be higher than a $\frac{1}{18}$th, and which Mr. Charles Stodder writes he has "ascertained to be a $\frac{1}{19}$th"? On page 30, vol. vii., of this Journal, Dr. Woodward has published a table of powers in decimals of an English inch. In this table he gives the *dry* Powell and Lealand $\frac{1}{16}$th at uncovered point the expression ·0623. That is all right as far as the dry front is concerned, but if the reader wishes to know the power of the immersion front at its lowest power he may substitute the decimal ·0533, and he will be not far from right.

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* See 'M. M. J.,' vol. vi., p. 203.

I can still furnish further evidence to prove my position, but do not think it at all necessary. The members of the Royal Microscopical Society can count the lines in the glass positives, which I believe they have duplicates of, and satisfy themselves of the truth or otherwise of my statements. I counted the lines with a low power, 4-inch, and positive eye-piece, and was very careful to count the same part of the same frustule in both cases.

Dr. Woodward brought out his tables in "fair play" to Messrs. Powell and Lealand, and undertook to show that the objective was *not* of higher power than its denomination. I write this communication in order to show that the immersion front made it of *much higher power*, and those makers whose objectives are correctly marked, or nearly so, would not have "fair play" if their work was tested against this objective and its actual power not stated.

V.—*On Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars.* By R. H. Ward, M.D.

The *nominal focal length* of an achromatic objective, as used by microscopists generally, represents its amplifying power as actually used in the compound microscope. Even the equivalency in amplifying power with a single lens of the same focus is no longer distinctly realized, while the size and appearance of the combination, its working focus, angular aperture, and microscopical efficiency, are not even hinted by the figures used. The nominal focus represents the magnifying power and those properties dependent on it. Like other measurements, these must be stated by comparison with known standards. To use diverse and unknown units of measurement in cases designed to be compared with each other is simply self-stultification. To call two lenses, of identical magnifying power, respectively ¼th and ⅕th inch lenses, is just as indefensible as to call two houses of equal height, 40 and 50 feet high respectively. To argue against the existing looseness of usage in naming lenses, is only to state what everybody knows in regard to the advantages of uniform standards of measurement generally. So impressed are many microscopists with the urgency of this question, and so determined are they to escape from some of the present confusion, that a committee has been appointed to report on the subject. Though that committee is unprepared to report, it is believed that giving publicity to some facts and opinions involved in the consideration, may lead to useful agitation and to increased definiteness of ideas and of information in regard to it. Of course it would be premature to claim or expect accuracy of statement or safety of opinion in such a complication of disputed questions; and what is said, is designed to be contributory and suggestive, and in no degree dogmatic or final.

The great variation in objectives of identical name is familiarly known and is undisputed. Among other people one-fourth of an inch is less than four-tenths and more than one-fifth; but among microscopists it may often be more than the first or less than the last. An indefinite number of figures might be published to prove or illustrate this irregularity, the writer having been particularly interested in making and recording these comparisons for more than a dozen years, and Messrs. Bicknell, Biscoe, Higgins, Cross, and many others having been especially interested in the same study; but it is idle to prove what everybody knows and admits. So familiar have some of these apparent errors become by use, and good usage too, that they have been often accepted as established, even one of the latest authorities * stating the power of the ¼-inch objective five times as high as that of the 1-inch.

In the early days of the compound microscope as a really useful instrument, we find microscopists wishing that microscope makers would "grind their glasses to some settled standard."† We are willing to be more reasonable now, or else the conditions stated have become more difficult. We do not desire, nor consider it practicable, that the opticians should make all their combinations of certain definite and conveniently-graded powers; but we do propose so to name our powers, if we can, that each number shall group together all those powers of which it is the nearest and best description.

Makers would doubtless be considered as doing a favour to those who use their instruments if they would, after finishing lenses, carefully estimate their powers and name them by the frac-

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* Suffolk, 'Microscopical Manipulation.' London, 1870.

† Baker on Microscopes. London, 1742.

tions most nearly representing those powers. But even if this were done, and much more now when this is certainly not done, or not done upon such a uniform plan as to be satisfactory, microscopists should always re-examine their lenses, in order to be definitely informed in regard to one of their most important properties.

The easiest method of examining the *magnifying power* of an objective, by measuring the image (of a known object) which it forms at a standard distance (now 10 inches), was as well understood a hundred years ago as now; a lattice of fine silver wire or of human hair, or a scale ruled on glass, being used to measure the image.* A positive ocular † or the eye-lens of a negative one is used as a simple microscope with which to read off the measurement. If a separate piece of apparatus were to be made for the purpose of measuring these powers, a positive ocular, with micrometer attached, would doubtless be preferred, it being placed by means of the draw-tube or some other contrivance at such a height that its micrometer should be 10 inches from the objective. Its reading would then give the real size of the image formed at that distance by the objective, and the ratio of this number to the known size of the object, say the distance apart of two lines on a stage micrometer, would give the magnifying power of the objective. But as few are possessed of a large variety of apparatus, or care to buy a piece for so infrequent a use as this, the measurement is generally made with an arrangement which every microscope ought to include—a negative ocular, with a micrometer in the focus of its eye-lens, whose advantages for general micrometry are so well understood, giving the best view of the object and a sufficiently good view of the measuring lines, that it is usually preferred for that purpose. Of course the field lens is removed in measuring the power of the objective alone,‡ but replaced for ordinary work. If it should be thought best to name lenses by their magnifying power alone, the power ascertained could be at once attached to the lens, the present 1-inch lens becoming No. 10, or x 10:§ but if it should be the usage to name it by its power when combined with some standard (say 2-inch) ocular, it would be marked No. 50, or x 50, or perhaps x 45 or x 55. Should it be preferred to retain the nomenclature by inches of focal length, a power of ten diameters might be called a 1-inch lens, and powers above and below rated in proportion. This plan is within reach of the opportunities of every microscopist, while the plan of actually employing a single lens of small aperture and exactly 1-inch focus as a standard of comparison is only adapted to the use of the opticians, and is not free from question as to what standard is meant after all. The lens made as a standard is probably not a 1-inch lens at all (principal focus), for the principal focus is never used in the microscope; and authorities differ as to whether it should have conjugate foci of 1 inch and 10 inches, or 10 inches apart (1 inch and 9 inches). Assuming x 10 as a 1-inch power, would be most easily applicable and unmistakable; and this power (ten), divided by the ascertained power of any ocular or objective, would give the equivalent focal length of that objective or ocular without comparison and beyond dispute.

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* The measurement of the image, formed by the objective only, on a screen at a distance of several feet, as employed by Dr. J. J. Woodward at the Army Medical Museum at Washington, is unquestionably the most reliable method of determining the amplifying power; but is a method which requires too many applications and too much skill to be universally applicable.

† The convenience and growing popularity in America of this Continental term suggest the propriety of its general substitution for the awkward name eye-piece.

‡ Dr. J. J. Higgins, in the 'American Naturalist,' Dec. 1870, p. 628.

§ It might be 9 or 11, and thus the various degrees of power would be conveniently expressed.

The chances of error in this case are the same as in ordinary micrometry, with one or two additions, and should in all cases be ascertained in order to test the reliability of any series of observations. They are due to the uncertain value of the divisions of the stage micrometer, to the like (but less important) variability of the measuring scale, to the uncertainty as to the exact optical correspondence of the lines selected for comparison in the two scales, and to the uncertainty as to obtaining exactly the assumed distance between the upper scale and a given point of the objective. The first of these errors is the largest, and its magnitude would surprise many who have noticed and admired the remarkable "perfection" of the common micrometers. A micrometer which ought to be the best in the writer's possession, with lines 100, 1000, and 2000 to the inch, has a certain error of ·02 and a limit of error of ·035. This is entirely too much latitude for a single source of error, and of course it is nearly eliminated by comparing a large number of spaces belonging to at least several different scales, rejecting any scales which by differing widely from the average standard are presumably erroneous, and averaging the rest. The remaining sources of error may be similarly reduced by averaging, though their aggregate limit of error, ascertained by comparing the average measurements with extreme figures beyond which there is no possibility of doubt, will be found to be very small and inconsiderable.

The standard *distance of measurement* in estimating magnifying powers may be stated to be, at present, 10 inches. The distance of 5 inches has been recommended, even somewhat recently,* and 8,† 9,‡ and 10§ inches have been successively used. The smaller number were evidently too small, and the last, 10 inches, seems to be permanently accepted as most correct theoretically and most convenient in use. If, however, the metric system were to come into general use, this distance would be changed to 250 millimètres, with increased convenience, and with a scarcely appreciable difference in results. The sooner such a change is made the better, provided it is certain to come at all, and possibly it might be considered only a fair concession to the convenience of the great number of Continental microscopists, and to the excellence of their metric system, to make this change without further delay.

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* Brocklesby, N.Y., 1851.

† Baker. London, 1742.

‡ Martin Fokes, Esq., P.R.S., 1742.

§ Lardner, Carpenter, Suffolk, &c.

The propriety of measuring the image at this standard distance, when estimating the power of objectives or oculars is undisputed, and it would seem equally indisputable that the whole power of the compound microscope should be obtained in the same manner, were it not that the authorities have always differed in regard to the subject. When Hooke, Griffith, Hogg, and other eminent authorities have directed that the image should be measured at the distance of the object on the stage, and Lardner, Carpenter, and Suffolk, in common with most microscopists, measure the image 10 inches from the eye wherever the object may be, it is useless to appeal to authorities. It would seem, however, that the former direction, to measure the image at the distance of the object, must be an advertency which could lead only to confusion. The writer has fully stated this question in a recent review,* and therefore omits further discussion of it here.

A more difficult question is as to the *point in the objective from which the measurement should be made.* If the objective had an optical centre and we could find it, there would be no difficulty in the case. But the modern objective has no permanent optical centre, at least none that we can easily find and use, and unless some one can give us a better rule, we may be obliged to measure from the bottom of the whole system, or from (about) the centre of the lowest pair or set of lenses. Mr. Charles R. Cross † has proposed to evade this difficulty by measuring 10 inches between the conjugate foci used, without regard to the position of the objective—a plan which would be very eligible with high powers, but inconvenient if not inapplicable with low powers, since few compound microscopes have a body short enough to bring the conjugate foci within 10 inches of each other with very low objectives, and, if they did, the magnifying power, instead of being that generally used, would be greatly reduced or altogether suppressed.

The very low-power objectives (say 4 and 5 inch) are usually mounted short in order to leave sufficient room between them and the stage, and their power, as ascertained by an arbitrary rule, would be greater than that at which they are usually worked, unless, in their ordinary use, the draw-tube were habitually raised enough to compensate for the shortness of their mounting.

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* The 'American Naturalist,' June, 1871, p. 229.

† Boston, 1870.

At what point of *screw-collar adjustment* the angular aperture and the magnifying power should be computed is one of the most complex questions involved in the discussion, and an entirely unsettled one. Most makers state the angular aperture of their lenses at its highest point, but no such uniformity of usage exists in regard to their magnifying powers.

With the lenses of a dozen years ago this would be comparatively unimportant, but with many of the high-power and high-angle lenses of the present day, the effect of the screw-collar movement is too great to be disregarded. It has been proposed, and would be most easy, always to rate objectives at their arrangement for uncovered objects, this being a naturally fixed point, and the only one at which the maker's judgment in regard to the accuracy of the correction is usually known: but this usage would greatly underrate many of the high objectives. On the other hand, rating them at their highest adjustment, or at an average between the two, might be vitiated by the fact that the point of highest correction is not a natural and fixed one, but is somewhat dependent on the judgment or caprice of the maker, some lenses of equal power being capable of a much larger range of corrections than others are. And finally, if we could agree upon some standard thickness of glass, and the glass were sufficiently uniform in refracting power, the same standard would scarcely be convenient for all powers (low powers being generally worked by the great majority of microscopists through glass, say $\frac{1}{100}$th or $\frac{1}{125}$th inch, for which many high powers are incapable of good adjustment), and few microscopists are sufficiently expert in the use of the screw-collar to make the same adjustment from the same glass-cover. Adopting the highest point of adjustment would perhaps involve the least change from present usage; and in cases of unusual interest or importance it might be well to give both extremes, or else to specify the angle and power at which the combination was worked to accomplish the results specified. Attention need hardly be called to the fact that this great increase of power and angle, amounting sometimes to more than one-half of the minimum amount, is due entirely, not to the interposition of the cover-glass or other medium, but to the change in the relation of the lenses caused by the movement of the screw-collar. Where an extra front of different properties is added, we have essentially another objective whose power and angle should doubtless be separately stated.

The use of *linear measurement* in recording and stating powers has become so general that there may now be said to be no respectable deviation from the custom. In the early history of microscopy, powers were generally stated, according to the visible flatness or depth of the object, in superficial or cubical measure, and it was plausibly urged that this represented the real, visible enlargement of the natural object; but, aside from the inconvenience of the large and often incomprehensible numbers thus obtained, this method gives in one sense the magnifying power, but in no sense the microscopical power employed. The power to see small things depends, so far as real or apparent size is concerned, on the distance from each other of minute points of structure, and this is in the exact ratio of the linear magnifying power. Squaring or cubing this power has acquired a suspicion of sensationism, if not of charlatanism, and is generally avoided in science.

If anything could be more confused and confusing than the different real and nominal powers of the objectives, it would be the corresponding powers of the *eye-pieces* or oculars. Made without any pretence of uniformity, and named without any serious attempt at significance, it has seemed until recently that no escape from the confusion was to be looked for. Yet it would seem to be convenient and altogether unobjectionable to have the oculars so named as to express their magnifying power, and the practice of doing this has been already introduced into this country. Some microscopists have renamed their oculars by their magnifying power, on the basis of one inch to ten diameters, and I am informed by Mr. Bicknell that Tolles has already adopted the same plan, in naming those of his manufacture, discarding the letter nomenclature (A, B, C, &c.) and selecting 2 in., 1½ in., 1 in., ¾ in., ½ in., $\frac{4}{10}$ in., and ¼ in., giving powers of 5, 7½, 10, 15, 20, 30, and 40 diameters. The writer has applied the same names to his oculars, applying the intermediate fractions $\frac{8}{10}$ inch, ⅔ inch, and ⅓ inch, to intermediate powers; and he is satisfied, by experience of its convenience, that this nomenclature only needs a trial, to secure its adoption by all who use the microscope for other purposes than amusement. Of course any microscopist, having determined the power of an objective and the powers of the microscope when that objective is used with his various oculars, can obtain the powers of his oculars by dividing the latter numbers by the one first named, and can then name his oculars, like the objectives, either by their magnifying powers or by their equivalent focal lengths. The rivalry of makers and the interests of trade are not involved in this case as in that of the objectives, and there may be no reason why this plan, if as acceptable to microscopists generally, as it has been to a few, should not come into immediate use.

In order to work the objectives and oculars at their standard powers they should be, of course, about 10 inches apart, either by length of compound body or by use of draw-tube; and it is believed that most objectives whose corrections are accurate enough to show any difference will work best at about this distance. Should a decidedly different distance be used in any observations of importance, it would be well to state that fact in recording the observation.

In reviewing this subject the following *points* would seem to be reasonably well *settled.* Objectives should be, and could be to a much greater extent than they now are, rated according to a uniform standard. They should be named not arbitrarily, but in a manner indicative of their magnifying power. Ten inches is the standard distance of measurement in estimating powers. This distance should be taken from the eye to the rule by which the measurements are made, without regard to the distance of the object on the stage. Magnifying power is always stated in linear measure. The magnifying power and angular aperture, as well as the maker's name, should be engraved on all objectives, and added to all particularly important drawings made by their means. Oculars should be named, like the objectives, in such a manner as to indicate their magnifying powers or equivalent focal lengths.

The following are some of the more important *queries* which still remain *open.* Should the standard 1-inch objective be characterized by magnifying ten diameters as used in the compound microscope, or should it be compared to a simple lens of actually measured focus or foci? Should the objective be named by its equivalent focal length, or by its amplifying power, or both? Should our standard distance of measurement be changed from 10 inches (254 millimètres) to 9⅝ inches (250 millimètres)? From what point in the objective shall the distance to the scale be measured? At what point of screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power? Should the name *ocular* be substituted for "eye-piece" in general use?—*The American Naturalist.*

VI.—*Notes on some Microscopic Organisms.*

By Prof. A. M. Edwards (U.S.A.)

The following observations I desire to have recorded, the more especially as, from all I can learn, they are unique, or, at least, have not been seen and published by anyone else. At the same time, I wish now to place upon record some discoveries connected with the life-history of certain animal organisms of minute size which I consider of importance and throwing considerable light upon a rather obscure portion of Biology, namely, the so-called subject of "Spontaneous generation." And I must say that I cannot but think that the use of this title, to designate the phenomena considered, is extremely inappropriate, and had best be discarded, as it does not properly indicate what is meant, and at the same time, I am sure, often prejudices persons in advance against even considering the record of experiments or published deductions. A really more correct name to use would be that proposed by Prof. Huxley, of Abiogenesis, until the whole matter has had a fairer hearing than there is a desire to accord it in certain quarters at the present time. Such plain and evidently truthful records as those of Pouchet and Bastian cannot be sneered aside by using the weight of any name, however worthy of honour and respect; and whatever the deductions to be drawn therefrom, the spirit of fairness, which should always influence the acts and words of truly scientific observers, demands, that no aspersions should be cast upon what they, or others working in a similar direction, may have already or should hereafter publish. I speak thus, for I have myself, in a degree, had to encounter much of this very unfairness, and I shall hereafter claim an immunity from it when I publish, as it is my intention to do, some experiments and deductions of my own, tending, as I hope, to assist in elucidating this interesting and important subject. In the meantime, and even aside from this connection, I desire to call attention to the value of the observations I have been so fortunate as to make, and which I now, for the first time, put into print.

It is my intention to make this communication as brief as possible, the more particularly as, although the observations described appear at first sight to be tolerably complete, yet, for my own part, I must consider them as but partial and fragmentary, and merely as memoranda of a phase in the hitherto insufficiently studied life-history of a group of plants a more thorough knowledge of which is of the utmost importance and interest. Therefore I thus set down what I have myself seen, intending to follow it up with more extended researches as opportunity offers. At the outset I would remark that the investigation of such phenomena as I now mention requires no more special apparatus than a tolerably good microscope furnished with a magnifying power of about four hundred diameters, and which is usually and conveniently obtained by employing a one-quarter or one-fifth of an inch objective on the usual ten-inch length of tube or body, and a B or No. 2 Ocular. But what is certainly required is a large share of patience and perseverance, and is, perhaps, exemplified by the fact that while studying this subject, as I have mentioned, at one time I remained at the microscope, only at long intervals removing my eye from it, for the greater part of three consecutive days. That is to say, certain points were observed about the middle of one day and all the rest of that day and evening; the whole of the next day and evening and nearly all of the succeeding day were spent in these investigations, watching the changes and transformations I am about to describe. And this was only at one time, for many hours thereafter were spent in the same way, and I feel sure that it is only by such earnest application that we can acquire any knowledge of the phenomena peculiar to what we know as life which will be of any lasting value.

The plant I examined belonged to the genus *Œdogonium,* of Link. The particular species was undetermined, and when it is considered upon what slight and evidently artificial characters species have been founded among the Cryptogamia, I am sure that I will be excused from venturing upon such delicate ground, especially as by so doing I run no danger of offending those who do not recognize the existence of species at all, or, on the other hand, puzzling those who do recognize the existence of natural species, but are not anxious to have them multiplied. The form observed is extremely common, growing in clear springs in various sections of the country, and is especially plentiful around the city of New York. To the unaided eye it appears as a bunch of fine filaments of a light green colour, and by the unknowing collector of microscopic wonders is usually classed under the inclusive title of "Confervæ." My first investigations were made in the month of April, a season when this peculiar phase of this plant appears to be common. And here I would impress upon students of nature, and more especially those who use the microscope to assist them in their investigations, the great advantage to be derived from making collections early in the spring. At that time the young stage of many organisms may be procured, and, as that season is also the time of rapid change and development, we are placed in the very best condition for watching transformations that may take place, and determining the true position of many forms which might be, and in many cases, I am convinced, have been ranked as perfect species. Let the young biologist remember that our knowledge of a species is never complete until we have made ourselves acquainted with the whole cycle of its existence from the germ to the adult, from the parent to the offspring; and the early spring in such latitudes as have a severe winter, during which almost all life lies dormant, is the time for such studies.

Up to this time the amount of knowledge bearing the portion of the life-history of the germs Œdogonium of which I desire to treat, is very incomplete; there being gaps which I think it will be found that what I have seen and have to describe, just fit into and fill. The perfect plant itself consists of an extremely fine, green-coloured filament, cylindrical in form, and having its frond divided at regular intervals by partition walls, so that the individual plant may be represented by a series of tubular cells or boxes, like tall pill-boxes, united end to end. The enclosing wall of these cells is itself colourless, and it would be difficult to see it, did it not possess a different refractive power upon light to the water in which it floats. It probably consists essentially of the same substance, chemically considered, as the enclosing cell-wall of the Desmidiæ, and commonly known as cellulose (a term, by the way, used in an extremely loose and indefinite manner), and is possessed of considerable tenacity, as will be seen to be the case farther on, and is evident by the fact that we can handle the filament quite roughly without tearing the cells asunder: thus it is common to find this plant growing in running water where it withstands the flow of the stream. The mode of reproduction known to exist is, as far as I can learn, by the shrinking inwards of the inner cell-membrane, commonly known as the "Primordial Utricle" of Mohl, and enclosing the cell-contents away from the tough cellulose coat, while, at the same time, the cell-contents themselves assume a more or less coarsely-granular condition, apparently from the enlargement of the individual particles of which it is made up. It is recorded that thereafter, at a certain period in this change, the outer cell-wall splits across at a point near to one end of the cell, and, while the lid so formed remains attached to one side the other and largest portion, the cell-contents escape from the cavity into the surrounding water, and gradually assume the spherical form. Thereafter there is developed upon one side of the sphere a ring of ciliæ which become more and more active until, at last, they move about with such energy that the little green globe assumes an extremely active motile condition, flying about for some time in such a lively manner that an observer meeting with it and unaware of its origin would certainly be inclined to rank it within the confines of the animal kingdom. In this state it has been called a "motile spore," but we will see, farther on, that it hardly can be ranked as an ovum in the ordinary acceptation of the word.

Up to this period our record, as hitherto published, is complete; but just here is a gap, and it has been my good fortune to make such observations as fill it and complete our knowledge of the life-history of this plant. It is known, however, that the motile condition just described is but transitory, and perhaps it may have been on account of the velocity of its movement, and the consequent difficulty of keeping it under observation, that the next step has not, as yet, been observed. In some unknown way, then, it has been supposed that the active spherical form assumes the static condition, develops rootlets or filaments, which serve to attach it to other objects, and then, being fixed, it thereafter by means of the usual well-known method of cell subdivision develops into a new filament resembling exactly the parent plant from which it sprung. As I have said, I have been enabled to show how the motile form is changed or, more properly speaking, developed into the static form, and, at the same time, I think, throw some light upon an important portion of microscopic biology. The changes and transformations which I herein record, I have seen not merely a few times, but perhaps thirty or forty, so that I am enabled to speak confidently as to the accuracy of my notes, as I have watched the whole process. It is as follows. At first the bright green-coloured cell-contents, around and investing which I have not been able to satisfy myself that I have seen a "primordial utricle," grow gradually coarser in texture by a process of differentiation of the mass in such a way that granules appear which increase in dimensions at the expense of the surrounding and investing substance, until the whole cell is filled with a coarsely-granulated mass, differing little in colour from the original cell-contents. At the same time the whole green mass recedes somewhat from the enclosing cellulose wall, and instead of filling it completely withdraws itself in such a way that its outline, near the ends, becomes rounded. This change is by no means slow, but, when the dimensions of the plant are magnified by the microscope, appears to be rapid. I should say, although I have not timed it, that this stage occupies from fifteen to twenty minutes in its accomplishment. Soon thereafter the cell-contents contract still more, moving towards one end of the cylindrical cell. Then, with a sudden snap, fracture takes place almost entirely across the tough cell-wall at a point about one-twelfth of its length from one end, that is to say a portion measuring about one-twelfth of the length of the whole cell splits across with a perfectly smooth and even fracture, still adhering by a very small portion, and looking like the lid to a box, is thrown back more or less, so as to expose the transformed cell-contents to the surrounding fluid. Occasionally but not commonly the fracture extends entirely across the cell-wall, and the two resulting portions become separated. Usually it occurs in the way I have described, so that after a while, and when all the cells of a filament have thus opened, it becomes transformed into a chain of straight portions bent usually in one direction in consequence of the dehiscence occurring for the most part along one side of the filament only. But now the green cell-contents move towards the openings thus formed, and slowly and steadily push themselves outwards, and, being elastic, escape after the manner that a small elastic sac filled with semi-fluid contents would escape from the hand if pressure were brought steadily to bear upon it. But in this case the emergence is not caused by the closing in of the cell-wall, but by a motile power resident in the elastic sac and its contents. It is not shot forth suddenly from the cell cavity, but squeezes itself out, and as soon as it has escaped assumes the form of a perfect sphere, and, as if exhausted by its previous exertions, comes to a rest. But although the whole mass is now in motion, the cell-contents do not remain at rest, but go on to another change. The bright green, coarsely-granular sphere being at rest, is seen first to become somewhat clearer upon the surface, and evidently a process of differentiation goes on by means of which a very delicate investing membrane is formed, but it is so delicate that it can only with difficulty be seen, and in fact can hardly be said to exist as a separate membrane. But upon one side is now seen to appear a bulging outwards of the mass until a nipple-like protuberance is formed, which however is not filled with the green matter formed elsewhere, but is clear and colourless. I have noticed that in most cases this protruding portion is formed at the point just opposite to the place in the cellulose cell-wall of the filament from which the spherical body has just escaped. But as this is not invariably the case it can hardly be considered as anything more than accidental. Soon thereafter there is seen to be a slight agitation going on upon the surface of the sphere, near to, and upon one side of the clear space. This movement then assumes a more definite character, and at last a moving cilia is seen to be formed or differentiated from the outermost portion of the globe. Soon another and another are formed in the same manner, attached by their bases around the clear space, and soon, that is to say in fifteen or twenty minutes, a ring of active ciliæ is seen to surround the nipple-like projection. As soon as they have all made their appearance, immediately they all begin to move together and in a violent manner, so that motion is imparted to the whole mass, and it swims about actively through the water. And this motion is so active that it is often difficult to follow the mass, and to keep it within the field of the microscope. For several minutes, or more rarely, for half an hour, this motion goes on, and up to this point I find that these changes have been observed and recorded by others. But now comes what I consider the most important part of this history.

It can be readily understood that, on account of its violent action, the further history of the spherical mass could not easily be followed, but fortunately on several occasions I saw specimens entangled in a mass of filaments in such a way that they could not escape from the field of view. Then I saw that the motion became gradually less and less vigorous, and at the same time the ciliæ disappeared one by one, melting out of view, being apparently absorbed again into the mass from which they were originally developed. At the same time one of them seems to elongate until it is one to three times as long as the diameter of the spherical mass, and also its point of attachment changes until we find it springing from the centre of the clear projection. While this is going on, the whole mass, not changing its position, assumes, however, a totally different outline. That is to say, the clear nipple-like blunt part remains about the same, but the opposite portion becomes pointed, while the intermediate space is inflate in such a way that the whole creature in outline somewhat resembles a trefoil clover. The inflated part, however, does not remain rigid, but soon subsides, while the clear end becomes more pointed, and now the creature is spindle-shaped in outline. As there is now but one cilium, it is by means of it that the creature moves about in an extremely active manner. In some, and in fact in by far the most cases I observed, the swelling of the middle portion is not fixed, but moves down the length of the creature towards what may with some propriety be termed the posterior extremity, as it is always projected backwards when it is moving through the water, and there disappears, to be soon followed by another swelling and wave-like projection, and so on. In fact, an action resembling very closely that seen to take place in the intestines of animals, and known as peristalsis, takes place. Many individuals move directly and straight onwards, preserving their body perfectly rigid, so that after a time the cell-contents are seen to arrange themselves in longitudinal bands. Others, again, revolve on their longest axes, and soon the cell-centents of these are seen to have arranged themselves in spiral lines corresponding to this movement. Some of these retain their straight condition, while others become bent around so as to form almost a complete circle, and then proceed onwards by a rolling motion. There is a regular and determined passage from one of these states to the other, as I was able to ascertain by careful watching; but the most remarkable fact connected with the whole matter is, that all of these forms are precisely similar to creatures which have been ranked by Ehrenberg and others in the animal kingdom, under different names, but most commonly that of *Euglena.* To make the resemblance of the Euglenas still more marked, as soon as the circlet of ciliæ has disappeared, and the mass elongates, a bright red spot appears near the clear end, and usually also, one or more clear seeming vacuoles are seen to arise within the green mass. The red spot has been called an eye, and the vacuoles stomachs; and in this way Ehrenberg was enabled to classify these forms as "Polygastric Animalcules." The spirally-twisted forms have been placed in a separate genus, and in fact I have seen, in the way mentioned, developed from the cell-contents of a filament of Œdogonium, forms identical with several genera of "Polygastric Animalcules." After a little longer time the cell-contents have again changed in appearance so as to be coarsely granular, each granule being so large and distinct that it can readily be distinguished, and now the active motion of the mass ceases, and it takes on the static condition. This it does by increasing in size, elongating and losing its cilium and red "eye" spot, while the clear portion elongates, subdivides, and branches out and becomes fixed either to a full-grown filament of Œdogonium or some other submerged substance that may serve it as a support. Now the cell-contents become finely granular again, and arrange themselves against the cell-wall, which is thickened considerably. Soon a bending in of an inner membrane or "primordial utricle" is seen to take place, and cell-division after the well-known method occurs, until a filament is formed exactly like that from which the original green sphere was projected.

The important points, then, recorded in this paper are the finding of the means by which the active spherical form is converted into the still state previous to growth into a filament. The most remarkable fact, however, is the identification of this phase with one or more of the forms which have been hitherto classed either in the vegetable or animal kingdom, most commonly the latter, according to the prejudices of the observer.

This note of the transformation of Œdogonium is the only one concerning the life-history of plants that I desire to record at the present time, but I have made so many detached observations very much of the same kind, that I wish to state that I am convinced it will be at some future day shown that all of the green, and some of the red-coloured forms similar to Euglena, and which have had several names bestowed upon them, are but transition states of fresh-water or marine Confervoid Algæ.

The second observation that I have to record is of certain phases in the life of animate organisms which have been commonly considered as belonging to the animal kingdom. But my notes here are more incomplete than in the case of the motile forms of the alga just mentioned, as it has been only within the last few weeks that I have seen what I am about to describe, and then only a few times, so that I wait for more opportunities for observation to confirm my experience. And here let me say that, apparently, the stages of change of these seemingly otherwise simple organisms I here record are, like the vegetable one just described, confined to the spring time of the year; and even then to a very few days. Of course these changes cannot be supposed to take place, for instance, within the space of one week, and in every individual in a single locality; but the changes are so rapid that it can only be by constant and patient observation that we may hope to see them occur, whilst slight modifying causes may defer or hasten the stage in different cases.

It was on one of the bright days during last spring that I collected in one of the pieces of fresh water in the Central Park in New York, a mass of matter made up of vegetable and animal material, but containing, as I knew, that which would yield material for observation and study by means of the microscope. Observing it, then, in that way, I was pleased to find in it numerous individuals belonging plainly to the group of organisms which have been grouped together under the head of *Amœba.* But remembering the observations of Dr. J. Braxton Hicks on the occurrence of amœboid forms in certain undoubted vegetables, I was of course unprepared to assign them to a position in the chain of life without further study. To decide this question if possible, and ascertain the origin of these wandering masses of protoplasm, I watched them at intervals for the better part of two days, and I saw the following changes take place. From an almost hyaline condition the *Amœba* became gradually more and more granular, the granules increasing in dimensions until the individuals appeared to be packed almost full of dense oil-globules. Then they came to a rest, or at least their hitherto lively movements were arrested, and presently near one end appeared evolved, so to speak, from the mass cilia, one after the other, until a crown of them was seen surrounding what was plainly now a defined locality. At the same time a change was going on all over the *Amœba*, by reason of which at last from this simple mass of albuminoid material a true ciliated animalcule, belonging, I believe, to either the genus *Kolpoda* or *Paramecium*, which resemble each other very much, was evolved.

The question at once, then, presents itself, Is not the amœba, in this case, the young motile condition of the ciliated animalcule? Such is my opinion. But we will require many more observations to decide whether, as I am of opinion is the case, they all pass through this condition, and also, at the same time, to throw more light upon a field in which I have spent much time in observation, namely, the origin of these simple organisms like the *Amœbæ*. At some future time I may take the opportunity of detailing what I have seen in this direction.—*Read before the Lyceum of Natural History, New York.*

PROGRESS OF MICROSCOPICAL SCIENCE.

The Development of the Sturgeon.—In a late number of the 'Bulletin of the Acclimatization Society of Paris' there is an account of the sturgeon's reproduction (sturgeon of the Volga), by Professor Owsjannikow. It seems that the sterlet (*Acipenser ruthenus*), the smallest of the Russian sturgeons, spawns in the Volga early in May on rocky bottoms, the temperature of the water being at 10° R. (= 54½° F.). The eggs are readily fecundated by the artificial method. After they have been in the water a few minutes they adhere to any object which they touch. The development of the embryo can be observed in progress at the end of one hour. On the seventh day they hatch. At first the young fish are $0^{\text{m}}{\cdot}007$ (about $\frac{27}{100}$ inch) long. At the age of ten weeks they are nearly two inches long. They feed on larvæ of insects, taking them from the bottom. Both in the egg and when newly hatched, the sterlet has been taken a five days' journey from the Volga to Western Russia, and in 1870 a lot of the eggs were carried to England to stock the river Leith. This species passes its whole life in fresh water. The other species inhabiting the Baltic, *Acipenser sturio*, *A. Huso*, *A. stellatus*, and *A. Güldenstädtii*, are anadromous. These species hybridize, and freely, and from this circumstance some Russian savants have pronounced them only varieties instead of species.

The Stomata of Leaves are well described for a couple of species in the 'American Naturalist,' by Professor T. D. Biscoe. In studying the development of these complex organs, he takes the youngest leaf of the plant, and finds on its base (the youngest portion) no trace or hint of stomata. A very little higher up he finds the epidermis appearing, many of the cells having built a partition across their front end, cutting off about a quarter of the original cells. These small cells are distinguished from the remaining portion of the originally single cells, and from the undivided cells, by being filled full of granular protoplasm, while the other cells are only partially filled with the protoplasm constituting the nucleus. These little cells, called mother cells, soon grow so as to become longer than broad, and are raised by the more rapid growth of the surrounding cells so as to leave an air-space below. An approach to a spherical form is now made by the mother cells, and the walls of the neighbouring cells are a little thickened with the deposition of cork substance giving the first trace of the thick-walled square of the ripe stomata. Next the mother cell divides by the formation of a thin partition which runs in the direction of the point of the leaf, and is perpendicular to its surface. Soon this partition thickens in the middle, and splits through the thickened portions to within about a fifth of each end. All further growth only effects minor changes in the form of the cells, or an increased thickening of their wall.

The Structure of Graptolites.—Professor Allman, in a recent paper, "Annals of Natural History," May, deals with the affinities of these animal remains. He thinks that certain of the projections upon them are the equivalents of the *nematophores* in living Hydroida.

The Double Markings of Triceratium are the subject of a paper by Col. Dr. Woodward, in the 'Lens' [last number], and are illustrated by an admirable Woodbury Photo-relief print, which brings them out with the most marvellous distinctness. These photographs we wish we could reproduce, for they certainly most fully bear out the remarks made by Dr. Carpenter in his work on the microscope, when he says that "in certain species of Triceratium, Coscinodiscus, and Actinocyclus the floors of the hexagonal depressions are studded with markings resembling those of Pleurosigma, and these are particularly conspicuous in the beautiful Heliopelta." Previous attention had been drawn to this circumstance by Stodder, Ralfs, and Reynolds. Dr. Woodward states that he has repeatedly verified the accuracy of the above statement for all the genera named, but limits himself in the present paper to a few remarks on a single species of Triceratium. Professor Christopher Johnston, of Baltimore, very kindly lent him, not long since, a slide containing a large frustule of *Triceratium fimbriatum*, on which the beadings, which with high powers can be made out on all the species of Triceratium, were remarkably distinct, and hence capable of being shown with a comparatively moderate magnifying power. The author then describes the method of photographing adopted by him for the two photos which appear in the illustration. He states that when the rows of dots seen in the picture of the broken frustule are examined by immersion objectives of the highest power and the best quality, they appear with white light as round garnet-red beads on a greenish ground, approximating in size and appearance the beads of *Pleurosigma angulatum*. He has endeavoured to obtain photographs of them with high powers, and sent silver prints of the results to the editor and the society. The comparative thickness of the frustules, and the marginal walls of the areolæ, interfere considerably with clear definition, with these high powers; still, he thinks an impartial examination of the photographs sent will dispel all doubts as to the real nature of these markings.

A Skin Disease (Molluscum contagiosum) resulting from a Parasite.—It is stated that Mr. Balmanno Squire thinks that he has discovered parasitic spores in the sebaceous matter which forms the pulp of the tumours in molluscum contagiosum. The spores, he thinks, are somewhat longer than those of the parasite of favus. Mr. Squire believes that the parasite causes, by the irritation it excites in the sebaceous glands, the increased secretion of sebaceous matter which characterizes this disease.

The Microscope in Entomology.—According to Mr. S. H. Scudder, who writes in the 'Entomologist's Monthly Magazine,' the microscope may be of advantage in the study of caterpillars—of course with low powers. But with such assistance he says that the shape and sculpturing of the head, the form of certain segments, and especially the precise number, location, and disposition of the spines, thorns, and hair-emitting warts of the body will be found to furnish abundant means of distinguishing the most closely allied and minutely subdivided genera.

Saccammina Carteri, a Foraminifer from the Carboniferous Limestone of Northumberland.—This discovery, which is in stated the valuable Natural History Transactions of Northumberland and Durham for 1871, we regret not having seen earlier. Still, as it is of interest, we lay it before our readers even now, as many of them may not have seen the volume which records it. It is a curious Foraminifer, and is amply illustrated by a very beautifully-executed plate in the end of the volume. A question appears to arise whether apparently independent specimens were originally connected together. Thus, says the author of the paper, Mr. H. B. Brady, it is a question whether these bodies represent individual animals, or to what extent they may have been connected with each other when living. It is not at all unusual to find on any weathered piece of the rock two segments connected by a stoloniferous tube—rarely three are found in this condition—and in one or two instances four or five have been noticed still retaining connection with each other. The bulk and weight of the segments and the comparative tenuity of the intermediate processes would be sufficient to account for the separation into single chambers, were this less constant than it is; but there is no need to suppose that the single segment may not represent a perfect animal equally with the many-chambered shell. Occasionally, though very rarely, a chamber is found with a round imperforate base and a single orifice at its apex, and if this is taken to correspond to the ordinary form of *Lagena*, the fusiform chambers may be regarded as analogous to the distomous varieties of that genus. The moniliform fossils might be compared to the *Nodosariæ*, but that all that have as yet been met with have an aperture at each end of the series of segments, and, for anything known to the contrary, the test might extend itself indefinitely in either direction. The test is composite and arenaceous, the constituent particles being fitted and cemented together so as to give a nearly smooth exterior. The size of the sand-grains and their mode of aggregation is a character of some importance amongst the recent *Lituolida*, but, as has been before stated, the process of mineralization has obscured the minute structure of the fossil in these particulars. The interior of the test is commonly smooth, resembling the recent *Saccammina;* but it sometimes presents a surface of very short, delicate, labyrinthic, shelly ingrowths. This cancellated or labyrinthic structure is often met with amongst the arenaceous Foraminifera, and in some genera it is developed to an enormous extent. Here and there a specimen may be found with a sort of circular patch on the surface, which has the appearance of a cicatrix resulting either from the gradual closing-in of an orifice or possibly the reparation of some injury to the shell-wall. These slightly raised concentric markings, apparently deposited regularly from without inwards, occurring frequently and with considerable uniformity, can scarcely be accidental. The positions in which they are generally noticed, *viz.* the sides rather than the ends of the segments, is an objection, though possibly not a fatal one, to the supposition that they mark the closure of normal apertures. He has named the genus after the same genus of Professor Sars, but the species after Mr. Carter, F.R.S.

Miasm viewed with the Microscope.—A series of researches, rather like those conducted some six years ago by Professor Salisbury, M.D., of Ohio, U. S. A., have been conducted by M. Bolestra, and recorded in the 'Comptes Rendus' of the French Academy. The water of the Pontine Marshes, and of similar malarious regions, he found to contain invariably, along with the common infusoria, a minute algoid vegetation, with an abundance of transparent, greenish-yellow spores, $\frac{1}{1000}$ mm. in diameter. This vegetation develops slowly in pure water and at low temperatures, but rapidly in the heat of the sun and amid decomposing organic material. It floats upon the water, giving an iridescent film when young, and its spores are found in the air near the marshes, and even at Rome, being most abundant in warm weather, and after a rain or during a fog, and least so in a cool, dry atmosphere. M. Bolestra regards these spores as the miasmatic agent in the production of the intermittent fevers for which the localities are badly celebrated.

A Conspectus of the Diatomaceæ.—Professor W. H. Smith has sent us the 2nd part of his 'Memoir on the Diatomaceæ,' which he purposes to go on with till it is completed. We observe with some satisfaction that the author has been at some pains to explain the more recent views of diatomacean structure in this part, which he did not do in the preceding one .Still, however, it is evident from the date of publication, that this was done before our previous remarks had come under his notice. He believes that all the frustules are "siliceous boxes," with either one portion (the cover) slipping over the other, as in *Pinnularia*, or with edges simply opposed, as in *Fragilaria*. If we take a frustule of *Melosira*, it may be compared to a pill-box—one portion slipping on to the other. The great majority of diatoms are thus constituted. It is perfectly evident that, in the case of the formation of a new valve, in the processes of self-division, this new part, which slips out from the older, must be somewhat smaller. Inside of the box is a membrane, inclosing the internal coloured or colourless substance, imbedded in which may always be seen, at least in the larger forms, a distinct nucleus, sometimes two, and sometimes a "germinal dot," with numerous fine threads radiating from the nucleus, or the germinal dot. As the frustule increases in width, one portion slips out from the other, and sometimes successive additions of siliceous matter are made to the edges of the box, somewhat analogous to the successive additions to the edge of the shell of a mollusc. When the widening of the frustule has reached a certain extent, the lining membrane at the places which would be exposed if the two portions were wholly to slip apart, infolds. He has reason to believe that, previous to this, a double membrane of extreme tenuity has been formed, commencing its growth *at* the nucleus (which itself is divided), and extended to the margins of the cell, which is thus divided into two nearly equal parts; for as soon as this infolding commences (perhaps now accelerated by the admission of water), the line of division can be seen progressing steadily inwards by the parting of this thin double membrane, so that in fifteen or twenty minutes the fissure is complete. He has, in very large *Pinnulariæ*, witnessed the whole phenomenon, from its inception up to the final self-division. While the *fissure* occurs in the short period of time named, to complete the *self-division* requires about six days. The part which slips out has, of course, one of the old valves upon it. Again, by self-division, the new valve becomes an old valve for the second-formed frustule; and so the frustules become smaller and smaller, as stated by Braun, 'Rejuvenescence in Nature,' Ray Soc., 1853, p. 132. The minute forms are, indeed, younger, or more newly formed, and, of course, more numerous. At this juncture, conjugation occurs, and a return to the normal condition of the original large frustule, by the formation of a sporangial frustule of double the size of the parent frustules. The following seems to be the author's admission of the peculiar elevations which undoubtedly exist in a certain number if not all cases. "As regards the terms "costæ" and "striæ," the distinction is altogether too vague to rely strongly upon them as characters; the term *pervious* costate, or striate, is sufficiently decided. Mr. Slack's researches appear to prove that even the coarsest costæ are simply very close or confluent dots. Only in the small forms, however, will any difficulty be experienced in deciding between costate and striate, as these terms are generally understood; probably here other characters will decide, leaving in these obscure cases the kind of marking as undecided."

Connection between Pyæmia and Bacteria.—At a recent meeting of the Pathological Society (May, 1872), Dr. Sanderson gave a series of demonstrations the object of which was, we believe, first to show the presence of bacteria in the infecting fluid of pyæmia, in the blood of living pyæmic patients, and in pyæmic abscesses; and, secondly, to prove that these living organisms were not introduced into the body from without, but were apparently produced within the body. It will be remembered that in certain experiments recorded in one of the reports of the Medical Officer of the Privy Council, Dr. Sanderson had shown that bacteria were introduced into fluids by contact with impure surfaces, and that when glass-rods, test-tubes, and the like, had been heated before being brought into contact with fluids, no bacteria were therein developed. Dr. Sanderson, we believe, had taken care in his experiments on pyæmia to avoid the introduction of germs from without into the peritoneal cavity. He first showed the liquid taken from the peritoneum of a guinea-pig twelve hours after the injection of fluid into it from the peritoneum of a kitten in which peritonitis was excited by the injection of peritoneal fluid. This liquid was swarming with minute bacteria, chiefly spheroidal, this being the feature of the liquids possessing the most intense infective character. The second specimen was the blood of an animal infected as the guinea-gig, and bacteria existed in the blood of the living animal taken from the heart. The existence of these organisms in the blood was regarded as the index of the general state of infection. The third fluid was that contained in the peritoneum of a guinea-pig twelve hours after injection into that part, of the subcutaneous fluid formed in connection with an abscess produced by the introduction of the solution of ammonia beneath the skin, the injection being made with calcined instruments, and with the special object of avoiding the introduction of external agents; and this fluid contained bacteria. A fourth fluid was exhibited, *viz.* the liquid from an abscess of the thigh of a puerperal woman, removed seven weeks after her confinement, the fluid being obtained through a fine canula into a glass tube, both of which had been superheated, the latter being at once closed. This fluid contained living organisms in it at the first moment of examination.

Is there Loss of Epithelium in Cholera?—The 'Lancet,' May 25, has an interesting annotation on this subject. The writer clearly holds that the epithelium of the intestine is not shed in cases of this disease. He states that in an inquiry into the subject there are two things that require to be kept apart, first, the post-mortem evidence of this loss of epithelium; secondly, its relation to the disease, supposing it to be an ante-mortem phenomenon. He does not contend that intestinal lesions, or this separation of the epithelium, are not commonly found after death from cholera; but he asserts that the doctrine which regarded these morbid processes as essential to the production of its characteristic symptoms is an unproven, if not altogether false one. If Dr. Cunningham's experience was founded on the thorough and accurate observation of even one fatal case in which there was found little or no detachment or destruction of epithelium, it is clear that it cannot be an essential phenomenon. It is scarcely to the point to urge that this writer's observations are based on a limited number of post-mortem examinations, seeing that in a series of microscopical examinations of the dejecta of cholera cases he failed to discover the presence of such epithelium, unless very exceptionally. It must be remembered, moreover, that the published observations of Dr. Lewis, his *confrère* in the scientific investigation into cholera instituted by the Government, point to exactly the same conclusion. "Judging," says Dr. Lewis, "from the cholera stools which have come under my observation in Calcutta—several hundred specimens—I believe that not more than two out of twenty slides will contain distinct traces of columnar epithelium." Considering the rapid transudation of fluid into the intestines, and the maceration which the mucous membrane undergoes, he would expect to find the epithelium separated after death; but if its rapid desquamation be the cause of the symptoms during life, the wonder is that any should be found in the intestines of a patient, and that it should not all have been washed away with the dejecta.

Phases in the Life-history of Monads.—The following extract from Dr. Bastian's, F.R.S., paper, to which we referred in our last, are full of interest. The changes which have been thus observed, says the author, referring to previous observations, constitute a very remarkable series, of which the following is a brief summary:—The simplest living units (*Bacteria*) first swarm in the infusion; these become aggregated at the surface so as to form a "proligerous pellicle," in which embryonal areas gradually appear: as a result of segmentation in these embryonal areas, specimens of *Monas lens*, $\frac{1}{3300}''$ in diameter, more or less suddenly make their appearance; they increase in size, occasionally assume an amœboid appearance for a time, and are ultimately transformed into real Amœbæ. The transition is effected by the loss of the flagellum, the appearance of vacuoles in their interior, and the simultaneous manifestation of polymorphism and a creeping mode of progression; at the same time a nuclear corpuscle develops in the interior, and the whole animal grows considerably. At last the Amœbæ gradually cease to exhibit their characteristic movements, whilst they become more or less spherical and motionless. Ultimately a firm bounding membrane is produced, and they pass into the encysted condition, in which, although slightly smaller in size, they constitute spherules $\frac{1}{1550}''$ in diameter. On the removal of some of this pellicle to the surface of a fresh infusion, the Monads and Amœbæ greatly increased in size; all the Monads gradually became converted into Amœbæ, and some of these at first went through the ordinary process of encystment, though at last (on account of some more sudden change in the fluid) they seemed suddenly to lapse into a morbid state. They were apparently unable to encyst themselves; and not being capable of continuing as Amœbæ, there sprang up in their interior a teeming progeny of new units (*Bacteria*), the production of which occasioned the final dissolution of the organisms in which they were evolved.

NOTES AND MEMORANDA.

The Best Mode of Carmine Staining the Tissues.—On this subject a valuable paper appears in the last number of the 'Lens,' by no less an authority than Col. Dr. Woodward. The writer specially alludes to carmine staining, and speaking of the supposed power which it gives of distinguishing living from dead tissues, he guards his readers against the hasty assumption that these important distinctions may safely be judged to indicate anything further than chemical differentiation. If the parts stained by carmine are entitled to be regarded as possessing any more significant vital properties, such as would justify the designation "germinal matter" bestowed upon them by Dr. Beale, and after him by so many English and American microscopists, this must depend upon quite other facts than their relations to carmine. Staining by carmine may be effected in a number of ways. The preparation originally employed was the simple ammoniacal solution, used first by Gerlach. It has subsequently been proposed to add glycerine or alcohol, or both, to the ammoniacal solution. Various formulæ of this character are strongly recommended in the books, and any of them can be made to answer. Thiersch, however, correctly pointed out, in his work on Epithelial Cancer, that the ammoniacal solution of carmine is not staple. First made, it stains less energetically than after a few days, when certain ill-defined de-compositions have commenced in the fluid; but as these progress its staining energy again diminishes. It has also certain disadvantages when used after other reagents. Thus, if applied to specimens previously coloured by silver, the black markings are promptly dissolved and the preparation is rendered useless. Thiersch therefore proposed as a substitute a rather complicated fluid, in which the ammonia was nearly neutralized by oxalic acid, and decomposition prevented by the presence of alcohol. This fluid was advantageously employed at the Museum for several years, but its proper preparation requires care and skill, and it gives no better results than the simpler method which will presently be recommended. In the same work, Thiersch advised for certain special purposes the use of carmine dissolved in a saturated solution of borax; and, with some modifications, this method is probably the most useful of any yet proposed. According to his formula, one part of carmine by weight is dissolved in sixty parts of a saturated solution of borax, the fluid mixed with twice its volume of absolute alcohol, filtered, and the filtrate used for staining. These directions have been quite extensively copied. It will be found, however, that the greater part of the carmine, crystallizing in some as yet unstudied combination with the borax, remains on the filter, and the fluid which passes through is comparatively pale, and stains but slowly. On the other hand, the crystals which remain on the filter, if dissolved to saturation in distilled water, yield a fluid which stains with great energy and uniformity. At first it might be supposed that the same quantities of carmine and borax solution might be mixed, and would answer every purpose. So, in truth, it would, when first mixed. On standing, however, like the ammoniacal solution, this mixture decomposes. Precipitation by absolute alcohol effects some change in its composition which secures its preservation unaltered for a considerable time. The solution made by dissolving the crystals just described in distilled water should therefore be employed. It should be filtered and transferred to a well-corked or stoppered bottle, in which it may be preserved several months without deteriorating, and will be found to be the most convenient carmine solution for staining.

Application of the Microscope to Petrography and Mineralogy.—We learn from the 'Neues Jahrbuch für Mineralogie,' &c., that Herr Rosenbusch observes under the microscope the action of pulverized minerals with reagents, such as disengagement of gas, gelatinization, &c., which show when these minerals consist of mixtures. The method of effecting various reactions which can be observed under the microscope is described.

A New Old Book on the Microscope.—We learn from America that a book which was first issued nearly twenty years ago has just come out in a new edition. It is Professor Brocklesby's 'Views of the Microscopic World.' We regret to learn from some of our American contemporaries, however, that it is in many respects an inferior book to the old one, that in point of fact it has not been brought down to the time. Most probably we shall soon have a copy of the work ourselves, and can then pass our own opinion on it.

CORRESPONDENCE.

Optical Curiosities of Literature.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *May* 24, 1872.

Mr. Editor,—In your May issue the Rev. S. Leslie Brakey, A.M., takes notice of the reply that I gave to the question of B. in the November, 1871, number, in regard to the angular aperture of the famous $\frac{1}{5}$th Tolles' objective belonging to the United States. The notice requires a rejoinder from me, if I will not submit to the imputation of a want of instruction, or in another word ignorance.

The notice reads, "In three of the cases the figures are no doubt correct; in the remaining one—the object immersed, uncovered—prompted by Mr. Tolles, he reports the aperture as 140° ! ! ! Colonel Woodward . . . . also answers for the four cases, but better instructed than his countrymen on coming to the critical case, he leaves out the figures." There are several things involved in the above passages to which I ask the attention of yourself and of your readers. First, he *asserts* what it was impossible for him to know, if it had been true, and which he had no reason or right to assert, that I was "prompted" by Mr. Tolles. In fact, the figures were taken from the record of the sale, July 19th, 1871, and were made from the original memorandum (now before me) of the maker, June 27th, and correspond with those of Colonel Woodward's measurement communicated to me in July. The three exclamation marks are original interpolations of Mr. Brakey, I surmise, intended to signify his "surprise." We will examine further on the equity of that surprise. Mr. Brakey does not assign any *reason* for doubting the angle, he leaves that to be guessed at. Now I will assure him again that the angular aperture of the lens at uncovered immersion is 140°, and 175° at maximum. And what will "surprise" him again, perhaps, is that Colonel Woodward's own answer, to which Mr Brakey pays a compliment, says precisely the same thing. Colonel Woodward's words are,* "on coming to the critical case" "Its angle is then 140°. Without further change of adjustment a drop of water may be introduced, and it is fitted for the study of uncovered objects wet." This is exactly what the reverend gentleman says Colonel Woodward left out, and precisely what I gave, taken from the original record of the maker, not prompted by Mr. Tolles after November. Does Mr. Brakey know that immersion objectives can be used on objects mounted dry as well as on those in balsam?

These quotations show the animus that prompted the gentleman's paper: but there is more yet. The original question of B., now acknowledged to be Mr. Brakey,† reads thus:—"It would not surprise me to hear that Mr. Tolles' fifth has an aperture of 150° or 160°." In November he would not be "surprised" by an aperture of 160°. In April he is astonished to the extent of three exclamation marks by an aperture of 140°. He has apparently made progress in his study of optics.

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* 'M. M. J.,' January, 1872, p. 28.

† 'M. M. J.,' Nov. 1871, p. 241.

When he writes the second chapter of the "Optical Curiosities of Literature" he can add his own lucubrations as the most ridiculous specimen of the Curiosities of Optical Literature.

I am happy to add that there are parts of Mr. Brakey's paper in which I heartily concur. I will mention his remarks (p. 224) on the ignorance of some of the working opticians of the science of their art, of the why and wherefore of the formulæ; and those on p. 229 on illumination and interpretation. These last cannot be too strongly impressed on the minds of all observers, whether expert or novices.

CHAS. STODDER.

N.B.—It is not my province to notice the attack on Mr. Tolles; I only wish to say that Mr. Tolles has not seen it, and it is not likely that he will for some weeks.

MR. BICKNEL AND MR. WENHAM.

To the Editor of the 'Monthly Microscopical Journal.'

CAMBRIDGE, MASS., *May* 29, 1872.

DEAR SIR,—On page 234 of the Journal for May, 1872, I find a communication from Mr. F. H. Wenham in an unusually venomous strain. I wish him to separate my identity entirely from that of C. S. Further, that I do not expect, *or want*, him to "apologize" for anything that I may have said; and also, to notice that the term "wilful deception" *does not* occur in any of my communications.

Very truly yours,
EDWIN BICKNELL.

To the Editor of the 'Monthly Microscopical Journal.'

BRIGHTON, *June* 5.

SIR,—Will you be kind enough to correct in your next number the following errata in the report you have given of my lecture or paper read before the Brighton and Sussex Natural History Society, "On the Natural History of Cure and Healing." The errors are important ones, as they render unintelligible *some* of the sentences in which they appear.*

I am, Sir,
Your obedient servant,
W. ADDISON.

CORRIGENDA TO DR. ADDISON'S PAPER.

P. 281, twelfth line from bottom, for "coats of the stomach"! read *coats of the new vessels.*

P. 282, fourteen lines from bottom, for "a fragile globular form or fibrous tissue," read *a fragile globular for a fibrous tissue.* Twelfth line from bottom, for "preliminary to granulation," read *preliminary* TO HEALING *by granulation.* Ninth line from bottom, for "extreme"! read *external.*

P. 283, between "local poisoning blood poisoning" insert *and.*

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* The proof was corrected by the Secretary of the Society, to whom as usual it was sent.—Ed. 'M. M. J.'

IMMERSION OBJECTIVES: THEIR APERTURE.

To the Editor of the 'Monthly Microscopical Journal.'

REGENT STREET, *June* 21, 1872.

SIR,—A contributor to the 'M. M. J.' of May, 1872, who seems to me to have an unworthy habit of introducing personalities, has picked up at second hand, without acknowledgment, a criticism of Dr. Royston-Pigott's,* which I should not care to notice, but that I am reminded by Mr. Jabez Hogg that I omitted to say a few words of explanation in answer to Dr. Royston-Pigott.

In October, 1868, I addressed a communication to the Royal Microscopical Society with special reference to the subject of immersion objectives. I ventured to suggest that "the limiting angle of refraction in water being about 48°, it follows that whatever is the degree of obliquity in the incident light on the object, the immersion objective never has to do with rays of greater obliquity than 48°." The "limiting angle" referred to is evidently the limiting angle of transmission between water and air, therefore I was referring to the condition of the rays from the dry object at their incidence on the immersion objective. That I did not say this specifically was doubtless the cause of Dr. Royston-Pigott's criticism. I may add that, save by Dr. Royston-Pigott and the contributor above referred to, I am not personally aware that anyone interested in the subject found a difficulty in interpreting my observation.

Dr. Royston-Pigott has not upon all occasions expressed himself with that accuracy which is desirable. For example, in 'M. M. J.' No. XIX.,† speaking of the rays emanating from a luminous point in balsam, he says that "every ray . . . . up to 62° 57′ will be able to reach the facet lens of the objective *viâ water*." But his own diagram,‡ however inaccurate it may be as a representation of an immersion front designed to transmit very oblique rays, is accurate as against this statement of his, for he exhibits several rays less oblique than 62° 57′ in the balsam which emerge into the water film and are there lost; they certainly are not drawn as "reaching the facet lens of the objective."

Again, in the same number of the 'M. M. J.,' p. 138, Dr. Royston-Pigott says: "*Whenever amongst several parallel plates the ray is examined, its direction in two identically refractive media is always found to be the same* [sic] *after any number of intermediate refractions.*"

In the standard works on Optics quoted by Dr. Royston-Pigott, namely, Griffin, Parkinson, &c., the direction of these rays is not described as "the same," but as "parallel." If I am not mistaken, "the same" means one thing, and "parallel" means another.

But these are only slight blemishes in these two interesting papers on "Immersion Lenses," which I should not have noticed but that I am advised that I am bound, if possible, to exhibit a weak point in my critic's own positions if only to show that I have read his attack on myself.

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* 'M. M. J.,' No. xxvi., Feb., 1871, p. 71.

† July, 1870, p. 23.

‡ 'M. M. J.,' No. xxi., Sept., 1870, facing page 134.

I think Dr. Pigott deserves the thanks of the Society for the arduous labour he went through in compiling his ready reckoner for angles of refraction.

I am, Sir, yours, &c.,
John Mayall, jun.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *June* 5, 1872.

Charles Brooke, Esq., F.R.S., V.P., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Library and Cabinet of the Society was read, and a vote of thanks passed to the respective donors. Among the recent donations there was a considerable collection of palates of Mollusca, accompanied by some beautiful drawings, sent by Mr. Mapleson, of Victoria, and a descriptive paper and a number of shells had now been received. Mr. McIntire had kindly undertaken to look over the objects, and see if the names were correct. The Secretary said the object last sent was a valuable addition, and he should propose a vote of thanks to Mr. Mapleson, which was unanimously accorded.

The Secretary also announced another present from Dr. Blake, of San Francisco, consisting of specimens of Algæ collected from hot springs in the States of California and Nevada. The Secretary read a letter from Dr. Blake, describing the conditions under which the specimens had been obtained. Mr. Roper, who had paid great attention to the subject, had kindly undertaken to examine the collection. A vote of thanks was then cordially given to Dr. Blake.

A paper was read from Mr. Roberts, of Rock Ferry, Cheshire, "On the Use of the Pentagraph in Connection with the Microscope."

A vote of thanks was passed to Mr. Roberts.

A paper from Mr. Cubitt, entitled "Remarks on the Homological Position of the Members constituting the Thecated Section of the Class *Rotatoria*," was taken as read.

A vote of thanks was then given to Mr. Cubitt.

Mr. Slack said, "At the last meeting of the Society Mr. Wenham read a paper on a new mode of illumination, which it was not possible to discuss properly, as no Fellow had had the opportunity of examining the plan. Since the paper was read, however, he had (and he believed Mr. McIntire also) examined the instrument. Mr. Wenham had contrived to enable observers to do with greater facility what was to be done, as he had previously explained, with a certain amount of difficulty and trouble, *viz.* by using the parabola and truncated lens. He (Mr. S.) had tried the new plan on several objects, and he had succeeded in obtaining some very striking appearances with butterfly scales lying flat on the glass and not on the cover. He had been able to get a beautiful definition of several diatoms. In one of the insect scales he noticed a striking exhibition of a complicated fracture running in a zigzag line. The fracture ran exactly between the spherical dots, which is the course cracks always take in diatoms. He could find not that any dot had been broken through either on the upper or lower surface. This mode of illumination did not, in his hands, succeed when the light passed through objects of considerable thickness, and possessing high refractive and reflexive powers. Such objects gave rise to internal reflexion, and a considerable amount of distortion ensued. He thought the instrument, nevertheless, to be a very valuable one, though it might not be applicable universally, for it gave information which could be obtained in no other way."

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* Secretaries of Societies will greatly oblige us by writing their report legibly—especially by printing the technical terms thus: Hydra—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. 'M. M. J.'

Mr. McIntire said he had tried the instrument (Mr. Wenham's illuminator) with considerable success and satisfaction. It could hardly be expected that the best effects should be obtained at the first trial, yet those he had obtained were very good, and practice had enabled him to acquire further skill in getting beautiful results. He had resolved, without much difficulty, *N. cuspidata*, *N. rhomboides*, *Surirella gemma*, and the Podura scales, all on a field more or less dark. The markings of the Podura scales came out with a distinctness which left nothing to be desired—a convincing proof of the accuracy of Mr. Beck's view as to their structure. The delicate cross-striæ on the test-scale which gave rise to Dr. Pigott's beads also occasionally came clearly in sight. A great deal of care was necessary in adjusting the object-glass and manipulating the illumination in order to ensure the best effects and to reduce the flare caused by too much light. With such objects as *Arachnoidiscus* and *Heliopelta* this was especially the case. It would be found also that objects mounted in balsam and illuminated by this apparatus could only be examined satisfactorily with dry lenses. He was altogether much pleased with the invention, and considered it an important addition to the limited resources at present at the disposal of microscopists in using high powers, whether for display or investigation.

Mr. Wenham said he wished to make some remarks relative to objects having bulk. These ought to have some extent of surface in contact with the glass. If in examining objects such as *Gorgonia* any means could be applied for increasing the surface of contact, the object would be displayed beautifully, but taken alone on account of its irregularities there must be only a few minute points in contact with the glass which abstract light partially, and in a way that might give false appearances. But if a fluid be introduced beneath the object, the light would be generally admitted and diffused throughout its structure. He would recommend that experiments be tried with hairs of animals or insects; for instance, a mouse's hair, which is a difficult object to show up well. If this object be laid on a total reflecting surface alone, without any intimate contact therewith, it would be invisible, but if it be wetted, then luminosity occurs; the water serving as a channel for conveying light into the object.

One preparer of objects had offered to mount diatoms and scales on the slides specially for this method of illumination if required.

Mr. Richards brought before the notice of the Society an apparatus, founded upon Mr. Stephenson's plan, for focussing under water. The difference between his own and Mr. Stephenson's apparatus was this: In the latter each protecting cap must be made to suit every size of object-glass, while in the former (Mr. Richards'), by means of three concentric screws, the nozzle of the cap can be attached to an object-glass of any diameter, intermediate or the smallest. In using the instrument for salt water the tube can be made of ebonite or brass, varnished.

This apparatus is more particularly adapted to the American arrangement brought before the notice of the Fellows in the January number of the Journal.

The Secretary announced that Mr. Ward had brought over two object-glasses, the latest production of Mr. Wales, the well-known American maker, and the Fellows spent a considerable time in the examination of these glasses.

The meeting was then adjourned until the first Wednesday in October.

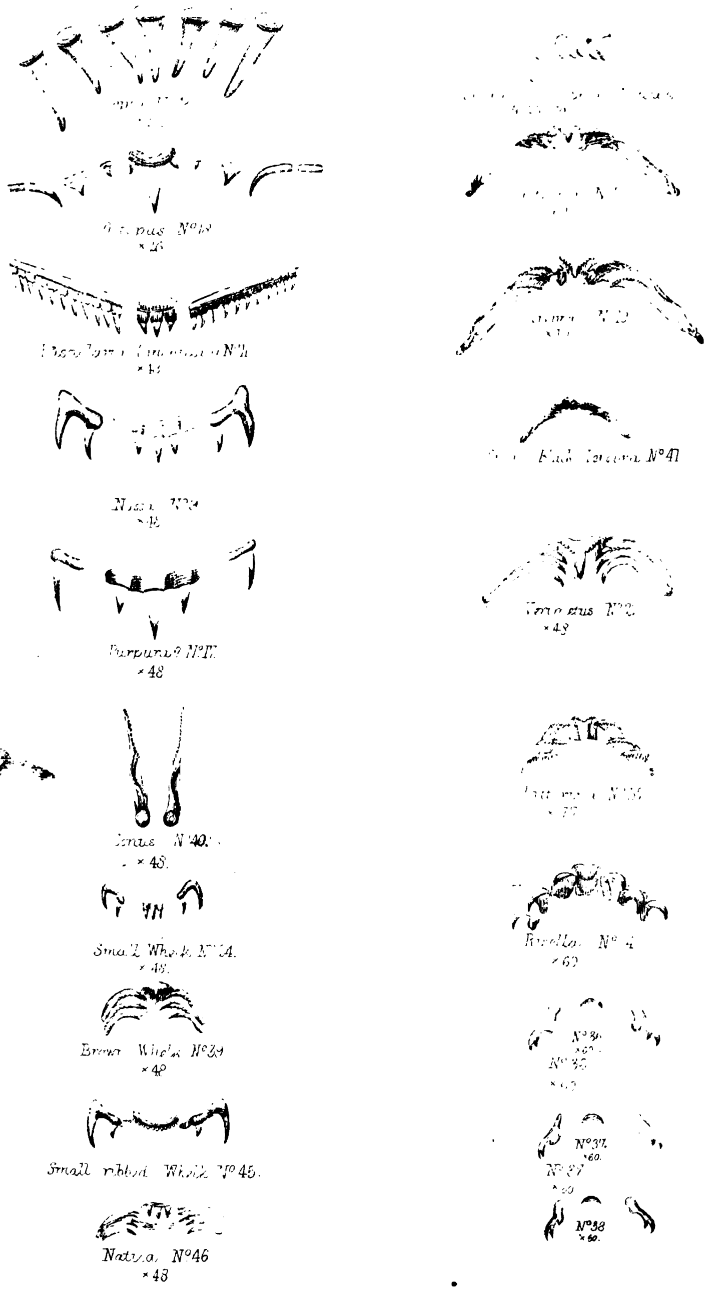
During the month of August the Library and Museum will be closed.

Donations to the Library and Cabinet from May 1st to June 5th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Quarterly Journal of the Geological Society, No. 110	*Ditto.*
Journal of the Linnean Society, No. 67	*Ditto.*
Proceedings of the Royal Society of Tasmania, 1870	*Ditto.*
The Lens, No. 2	*Editor.*
The English Flora, Vol. V. Part 1st comprising the Mosses, Hepaticæ, Lichens, Characeæ, and Algæ. Edited by W. J. Hooker, 1833	*W. W. Reeves.*
Conspectus of the Families and Genera of the Diatomaceæ, Parts 1 and 2. By Professor H. L. Smith	*Author.*
Notes on New and Rarer Species of Diatomaceæ of the U. S. Seaboard. By F. W. Lewis, M.D.	*Prof. H. L. Smith.*
On Extreme and Exceptional Variations of Diatoms in some White Mountain Localities, &c. By F. W. Lewis, M.D.	*Ditto.*
On some New and Singular Intermediate Forms of Diatomaceæ. By F. W. Lewis, M.D.	*Ditto.*
Bulletin de la Société Botanique de France. July, 1871	*Society.*
A slide of Foraminifera from the River Dee	*Thos. Shepheard, Esq.*
Fifteen Slides of Starch Granules	*J. Needham, Esq.*

John Emanuel Winspear, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.



W. West [illegible]

Molluscan Palates.

Reduced to ⅔ of size of Drawing.

THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1872.

I.—*Notes on Victorian Mollusca and their Palates.*

By Charles M. Maplestone.*

(*Taken as read before the* Royal Microscopical Society, *June* 5, 1872.)

Plates XXV., XXVI., and XXVII.

Octopopidæ. *Octopus*, No. 18.—Not common; seems identical with *Oct. vulgaris*.

Palate, formula, 3.1.3. The teeth do not agree with those drawn in Mr. Hogg's paper, 'M. M. J.,' March, 1868. The central tooth has two lateral denticles. 1st lateral is much smaller. The 3rd is not so large. The bases of the teeth are also different.

Sepiadæ. *Sepia*, No. 43.—Common. Shell and animal seem identical with *Sepia officinalis*.

Palate, 3.1.3. The teeth are all very much alike. The 3rd lateral wants the expanded base which Mr. Hogg gives it, and in

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* The subjoined letter accompanied this paper:—

No. 4, Josephine Terrace, Albert St., East Melbourne,
February 20, 1872.

Sir,—Having made a collection of the palates of Mollusca while resident on the sea-side of our harbour, and having been in correspondence with Mr. S. J. McIntire on the subject, at his suggestion I beg to forward you by this mail (as a registered packet) drawings of the same, together with some notes on them, that may be of interest, and deemed suitable for the pages of the 'Monthly Microscopical Journal.' The drawings are a first attempt *with a pen*, and I trust they will be found capable of reproduction without much difficulty.

I have made the notes as concise as possible, perhaps too much so, and have arranged them according to the Families, as far as I am able. I have throughout retained the numbers that I gave them in the order of collection, as my slides and shells are so numbered; but of course if they are inserted in the Journal it will be better to substitute those of the figures of the plates in the order in which they may appear. I will forward the shells to you, and will advise you of the means by which I do so before closing this, hoping you will be enabled to place them in the hands of some one who will be kind enough to name them correctly, and which names may I ask you to insert in their places, and to send me a list with my numbers added, so that I may with certainty know the proper names.

Although I have not drawn the palates, I send also some fresh-water shells, of which No. 48 is one that I can find no genus in Woodward that I can include it his figure the well-marked longitudinal muscular bands are not shown. Specimen in cabinet of R. M. S.

MURICIDÆ. *Triton* (?), No. 35.—Long, mottled, spiral shell. Varices small, irregular. Hab., seaweed below low-water mark. Very rare; only found one or two specimens alive; dead shells common. Operculum horny.

Palate, 3.1.3. Median reflexed, denticulated with large central tooth. 1st lateral, with a slight spinous process above the tooth. Specimen in cabinet of R. M. S.

Fasciolaria (*monilia?*), No. 11.—Animal bright red in colour. Operculum horny, claw-shaped, but large enough to close aperture. Hab., below low-water mark. Scarce.

Palate, 1.1.1. The median having three denticles, the bases of which are finely furrowed. Laterals 11 denticles. It is a remarkably clear and regular palate.

BUCCINEADÆ. *Nassa* (?), No. 9.—This is the common dog whelk, occurring plentifully between tide marks, and is a voracious feeder on dead and living molluscs, crabs, &c. A perfectly-formed shell is rare, the apex appearing to suffer from abrasion. It is variously marked. Some are reticulated or chequered; in others the dark markings form lines, and occasionally all trace of marking is lost. Operculum horny, obovate apex at end.

Palate, 1.1.1. Median, three denticles of equal length; laterals with basal tooth. Specimen in cabinet of R. M. S.

Purpura, No. 17, probably *P. textilosa*.—Hab., near low water.

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in. It is a sinistral shell, and has longitudinal ridges. I trust I am not asking too much of you in this matter; my anxiety to place the palates, &c., before the readers of the Journal in as complete a form as possible must be my excuse.

I enclose with the shells a small packet for Mr. McIntire, which please be kind enough to hand to him.

I am, Sir,
Your most obedient servant,
CHAS. M. MAPLESTONE.

WALTER W. REEVES, Esq., *Assistant-Secretary*,
Royal Microscopical Society, *King's College*, *London*.

Mr. Henry Woodward has seen the shells, No. 48, and kindly written the following note about them.

WALTER W. REEVES.

BRITISH MUSEUM, *July* 5, 1872.

DEAR SIR,—I have now the pleasure to return you the little shells. I only this very morning found leisure to compare them, having been very much pressed for time. They are the young of a fresh-water shell named *Physa aliciæ*, var. Lovell Reeve. In our case the adult attain to nearly four times the size.

Yours very truly,
HENRY WOODWARD.

WALTER W. REEVES, Esq.

Very rare where I made collection, but common in other places. Operculum lamellar, horny.

Palate, 1.1.1. Lateral has no basal tooth. Median, three denticles, middle one longest. Specimen in cabinet of R. M. S.

Purpura (?), No. 24.—Small whelk, purple inside. Hab., near low water on serpulæ-covered rocks. Common. Operculum claw-shaped, horny.

Palate, 1.1.1, very like that of *Nassa*, but much smaller. The denticles of the laterals are long and thin. Specimen in cabinet of R. M. S.

Purpura (?), No. 39.—Brown whelk, similar in shape to the last, but the mouth was thin and semitransparent. Shell covered with a dark brown epidermis, which was produced into irregular longitudinal short fringes. The palate is different from *Buccineadæ*. I have no specimen of shell. The only one I met with was transmitted to the late Mr. Hall; it may probably belong to *Muricidæ*.

Palate, 3.1.3. Median reflexed, denticulated. 1st and 2nd laterals slightly denticulated. 3rd smooth.

Purpura (?), No. 45.—Shell transversely ribbed; inner lip callous, extends over large portion of whorl; outer lip thickened. Possibly *Nassa thersites*. Hab., low water. Specimens were found in enclosure for graving dock.

Palate, 1.1.1. Central tooth lunate, with numerous round pointed denticulations. Lateral teeth with a basal tooth and small flat processes on inner side.

CONIDÆ. *Conus*, No. 40 (*C. punctatus?*).—Shell with thick dark-brown epidermis. Marked with irregular brown spots. Column plicated at lower portion. Animal pink, with black eyes. Hab., near low water. Only found on two occasions. First a single specimen, from which I failed to obtain the palate. Second, when there was an extra low tide and calm weather, when I obtained several.

Palate, 1.1. Teeth long, serrated near tips. Bases orbicular, very difficult of dissection.

NATICIDÆ. *Natica*, No. 46.—Hab., sand near cockle bed between tide marks. Very rare. Operculum large, paucispiral, horny; jaws spiny; very large mouth; short foot.

Palate, 3.1.3. Median reflexed; three denticulations. 1st and 2nd laterals, claw-shaped, bifid. 3rd lateral simple.

Note.—On drawing this shell in my diary I find there is another (probably a Naticina) that I had erroneously considered but a variety of this.

Natica (?), No. 26.—Small striped marine snail. Abundant.

Hab., sand near high-water mark. Has very large mouth; apparently feeds on animal matter contained in the sand, which abounds with live foraminifera, as I found the intestines full of it. Operculum horny, subspiral; shell snail-shaped, one or two dark brown stripes. Palate consisting of rows of simple sickle-shaped teeth, altogether different from any marine form I have seen. Specimen in cabinet of R. M. S.

CERITHIADÆ. *Cerithium,* No. 7.—Hab., sand, generally partially imbedded, between tide marks, nearer high than low water. Rare. Operculum horny, multispiral.

Palate, 3.1.3. Median reflexed; central denticulation much the longest. 1st lateral with slightly rounded claw, edges partially reflexed and finely denticulated; 2nd and 3rd, edges reflexed, denticulated. Specimen in cabinet of R. M. S.

Cerithium (?), No. 10.—Shell granulated. Hab., stream on sand, nearer high-water mark than low. Operculum horny, multispiral.

Palate, 3.1.3. Median reflexed. Five denticulations, central longest; base articulated. Laterals, edges reflexed, denticulated. Specimen in cabinet of R. M. S.

TURRITELLIDÆ. *Turritella* (?), No. 41.—This is smaller than the last described. It has not a canal; peristome entire, longitudinally furrowed. Hab., sand between tide marks.

Palate, 3.1.3. Median reflexed; three denticulations, not articulated. Laterals reflexed, denticulated.

Vermetus, No. 22.—Only found one specimen, in the root of a large mass of kelp, but have lost it, so cannot describe it at all.

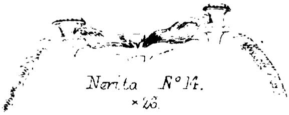
Palate, 3.1.3. Median reflexed, denticulated, central one very long. 1st lateral, long tooth at end, lower edge denticulated. 2nd lateral tooth-shaped, slightly denticulated. 3rd similar, but not denticulated.

LITTORINIDÆ. *Littorina,* No. 52.—Shell smooth, generally corroded; lip slightly everted. Hab., between tide marks at Schnapper Point; operculum horny, paucispiral.

Palate, 3.1.3. Median reflexed, central denticulation truncated. 1st and 2nd laterals, upper edges reflexed and denticulated; 3rd has much longer comb-like denticulations.

Risella, No. 4.—Shell with irregular keels more or less developed. Hab., between tide marks, nearer high than low water. This is the common dog periwinkle, so called from not being edible: it is carnivorous. Operculum paucispiral.

Palate, 3.1.3, very long. Median reflexed, with broad, central denticulations. Laterals reflexed. Specimen in cabinet of R. M. S.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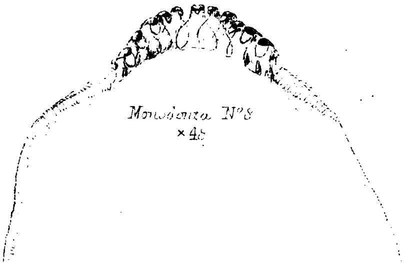
Monodonta No 8
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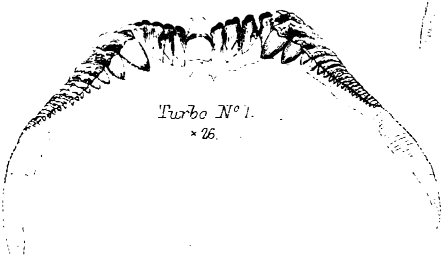
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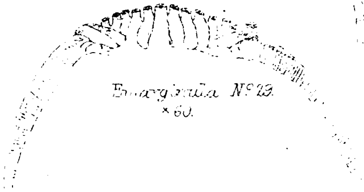
Emarginula No 29.
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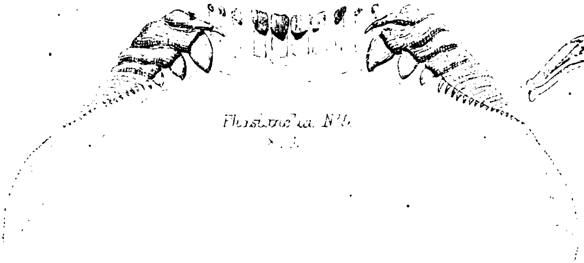
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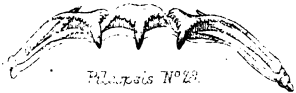
Pileopsis No 28.
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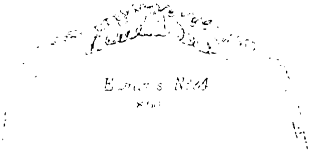
E[illegible]s No 44
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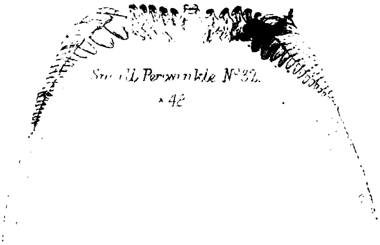
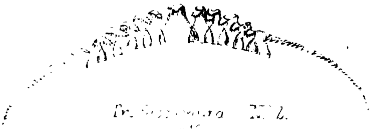

Small Periwinkle No 32.
×42


Pr[illegible]ra No 2.
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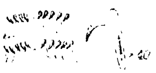
Doris No 16
×20

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Molluscan Palates

[illegible]

Risella (?), Nos. 36, 37, and 38. *Rissoa* (?).—Hab., low water, on seaweed on rocks; only found on one occasion. They may be merely varieties, but I found the following distinctions:—No. 36, brown, lip toothed, pink inside. No. 37, larger than Nos. 36 and 38, brown marked, lip toothed, purple inside. No. 38, smooth lip; thin, semitransparent, showing brown markings through.

Palates, 1.1.1, very similar, but I think the figures sufficiently indicate, in conjunction with the shells, a difference of species. Median crescentic; laterals with two denticulations. In No. 36 the base is much dilated; in No. 37, less so, and brought almost to a point at the back; and in No. 38, slightly dilated and more like No. 36. Specimens of Nos. 36 and 37 in cabinet of R. M. S.

NERITIDÆ. *Nerita*, No. 14.—Hab., crevices of rocks near low water; rare; dark bluish black in colour; operculum calcareous, paucispiral inside callous, and black outside, articulated. Posterior end of animal much shortened; appears to absorb internal whorls.

Palate, ∞.3.1.3.∞. Median small, toothed. 1st lateral broad, subtrapezoidal, upper edge reflexed and denticulated near outer portion. 2nd, very small, almost hidden. 3rd, with large prominent head or cap, nearly semicircular, edge crenated. Uncini numerous, reflexed, toothed; outer ones serrated.

TURBINIDÆ. *Turbo* (*Lunella?*) *undulata*, No. 1.—Two to three inches in diameter, deeply umbilicated. Operculum calcareous, subspiral inside, callous outside. Hab., seaweed on rocks at low water. Called "Warrener." Edible. Common often bears Pileopsis, No. 28, parasitic.

Palate very large and beautiful. ∞.5.1.5.∞. Median reflexed slightly, smooth with process above. Five laterals reflexed, crenated, outer edges of bases expanded, overlapping at sides. 1st unc. small, claw-shaped tooth; 2nd with large, smooth tooth, sides crenated; the others gradually diminishing in size, gradually changing into a ligulate form, with ends slightly reflexed. Specimen in cabinet of R. M. S.

Phasianella, No. 5.—Hab., low water and below, rarely seen above; live shells rare, dead ones plentiful in some parts. This shell is most beautifully marked with crescentic-shaped patches of various shades of pink, red, purple, brown, and yellow, forming different patterns; the crescents sometimes running together and forming irregularly-shaped markings; in others more regular, and in longitudinal rows, generally close together, but in varying distances and sizes, down to very fine longitudinal lines in which the crescents are very small. Occasionally marked as in the drawing in Woodward's 'Manual of Mollusca,' but rarely. It is very rarely found perfect; even when alive the lip is very brittle.

Palate, ∞.5.1.5.∞. Median doubtful, but think I have occasionally seen it, very small; laterals reflexed plain. 1st Unc. claw-shaped rugose. 2nd, large smooth tooth, sides rugose, others similar, diminishing in size and changing to long-curved serrated teeth, very numerous. Slide in cabinet of R. M. S.

Elencus, No. 34.—The base is not so flat as the figure in Woodward. Hab., low water; very rare; green pearly lustre inside. Operculum horny, multispiral.

Palate, ∞.5.1.8.∞. Median reflexed, denticulated. Laterals with ovate teeth, denticulated; bases overlapping at sides. Uncini hooked, very numerous. Specimen in cabinet of R. M. S.

Trochocochlea, No. 2.—Hab., rocks between tide marks. The common periwinkle, gathered with No. 8, as the same. Shell with rugose longitudinal furrows, marked in larger specimens similar to *T. zebra*, which this may be. Inner lip with a slight tooth. Operculum horny, concentric.

Palate, ∞.5.1.5.∞. Median reflexed; laterals, upper portion reflexed, ligulate, denticulated; bases broad, overlapping. Uncini hooked, serrated; very numerous. Specimen in cabinet of R.M.S.

Monodonta (?), No. 8.—Hab., rock between tide marks. The difference between this and Trochocochlea is in being smooth, no furrows, and a rather flatter shell. Foot of animal is much longer. Operculum concentric, horny. Inner lip with a slight tooth. Sometimes bears Limpet, No. 30, as a parasite.

Palate, ∞.5.1.5.∞. Median longer than Trochocochlea; laterals have a smooth central tooth; bases overlapping at sides. Unc. hooked, serrated; very numerous. Specimen in cabinet of R. M. S.

Monodonta (?), No. 32.—Small flat periwinkle with calcareous subspiral operculum. Hab., rocks and seaweed below low water; rare; only found on one occasion. Shell flatter than the previous two, with wavy, transverse, slightly-elevated ridges.

Palate, ∞.5.1.5.∞. Similar in character to Turbo and Phasianella; median inflexed at top and sides; laterals reflexed, with very broad, wrinkled, overlapping bases. Unc. with large teeth gradually diminishing, and changing into hooked, serrated teeth; very numerous. Specimen in cabinet of R. M. S.

Monodonta, No. 33. (Monodonta ?).—Small periwinkle, rough, longitudinally granulated; rare; only found on one occasion. Operculum horny, concentric. Hab., seaweed on rocks below low water.

Palate small, ∞.5.1.5.∞. Median reflexed, denticulated, with very broad, expanded base; laterals the same, with overlapping bases. Uncini hooked, serrated; very numerous. Specimen in cabinet of R. M. S.

HALIOTIDÆ. *Haliotis*, No. 15.—Hab., rocks at low water; only found on one occasion, but more plentiful in other places. Specimens were five and six inches across; animal weighed 14 oz. without shell.

Palate very large and fine, ∞ . 5 . 1 . 5. ∞ . Median small, reflexed. 1st lateral subtrapezoidal; upper edge reflexed. 2nd, reflexed. 3rd, with large tooth, very rugose. 4th and 5th, similar but smaller. Uncini hooked, finely serrated; numerous. I have reckoned the 3rd, 4th, and 5th laterals to be such as they are, very different from the Uncini. Specimen in cabinet of R. M. S.

FISSURELLIDÆ. *Emarginula*, No. 29 (Hemitoma ?).—Liberty cap limpet. Hab., rocks near low water; not common; very delicate, ribbed, white shell.

Palate, ∞ . 5 . 1 . 5 . ∞ . Median and laterals broad, slightly reflexed, crenated; basis overlapping to a small extent. 1st Unc. very large, with large irregular tooth; others small, hooked; numerous.

CALYPTREADÆ. *Pileopsis*, No. 28.—Hab., parasitic on Turbo, never found by me in any other position; it sometimes bears small ones parasitic on itself; it nurses its young, which have spiral shells, and wears away the shell of its host slightly.

Palate, 3 . 1 . 3. Median reflexed, denticulated, middle one very long. 1st lateral denticulated, one of which is produced into a long tooth. 2nd and 3rd partially reflexed and denticulated; 2nd but slightly. Specimen in cabinet of R. M. S.

PATELLIDÆ. *Patella*, No. 3.—Large striped limpet, one to two inches across, occasionally larger. Hab., rocks near low water. Shell pearly inside; radiating stripes of dark colouring.

Palate very long, from five to six inches even in the smaller ones. 3 . — . 3. 1st lateral reflexed, apparently articulated. 2nd lateral reflexed, bifid at extremity. 3rd indistinct, probably two small teeth. Specimen in cabinet of R. M. S.

Patella, No. 21.—Small deep limpet, not common. Hab., near low water.

Palate, 3 . — . 3. 1st lateral, long tooth at top, with a basal tooth. 2nd ligulate. 3rd very small, and apparently composed of two very small teeth placed close to the base of the 2nd. Specimen in cabinet of R. M. S.

Patella, No. 25 (longicosta ?).—Not common; porcellaneous inside; shell ribbed, but not quite so prominent as Woodward's drawing of *P. longicosta*. Hab., near low water.

Palate, 4..4. 1st lateral, with basal bifid tooth. 2nd ligulate, very slightly reflexed. 3rd and 4th ligulate reflexed.

Note.—This is the only palate of the limpets which clearly shows four laterals. Specimen in cabinet of R. M. S.

Patella, No. 30.—Rare; very small deep limpet; found by me only parasitic on No. 8; smooth, with few brown markings near edge.

Palate, 3. – .3. 1st lateral, with basal bifid tooth. 2nd and 3rd ligulate; indistinctly divided. Specimen in cabinet of R. M. S.

Patella (*Acmæa ?*), No. 20.—Small flat limpet, not common; dark brown markings.

Palate, 3.3. 1st lateral, with basal bifid tooth. 2nd lateral ligulate, with a basal tooth. 3rd ligulate. These two are better seen in the side view. Specimen in cabinet of R. M. S.

Siphonaria (*aspera ?*), No. 27.—Nutmeg limpet. Hab., near low water; rare; but more common in other places.

Palate, ∞.1.∞. Median very small, laterals hooked, with double points. Side view gives better idea of the form than front view. Specimen in cabinet of R. M. S.

CHITONIDÆ. *Chiton spinosus*, or *Acanthopleura* (?), No. 6.—One to four inches long; common; edges covered with spines. Hab., rocks near low water.

Palate, 3.4.1.4.3. Median reflexed. 1st lateral slightly reflexed. 2nd lateral long, with large trifid head. 3rd, ligulate, slightly reflexed. 4th, long, claw-shaped. Three outer ones (Uncini ?) flat, very slightly reflexed. Specimen in cabinet of R. M. S.

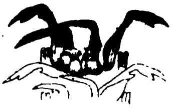
BULLIDÆ. *Bulla* (*oblonga ?*), No. 47.—Hab., below low-water mark; only found alive in enclosure for graving dock; has three chitinous gizzard teeth.

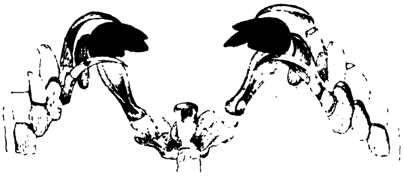
Palate, 3.1.3, very large. Median broad, with a small central denticulation, and eight large lateral ones. 1st lateral, dark teeth, with light large denticulations. 2nd similar, but first denticle is bifid. 3rd triangular, slightly reflexed.

Philine (*aperta ?*), No. 23.—Hab., in sand below low water; only found an occasional solitary specimen, except in enclosure for graving dock, where large numbers of fine ones were exposed. Two to four inches long, three calcareous gizzard teeth.

Palate, 1.1. Teeth claw-shaped, denticulated on inner margin. Plate XXVII., Fig. 23, *a* shows side view; *b*, front view; *c*, view from intermediate point. Specimen in cabinet of R. M. S.

DORIDÆ. *Doris* (?), No. 16.—Hab., low water on rocks; very rare. Limaciform; tentacles two; simple truncated narrow cones.


Patella No 3
×26.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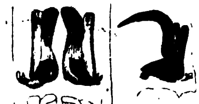

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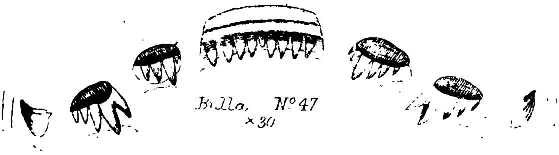
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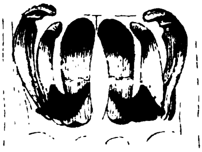
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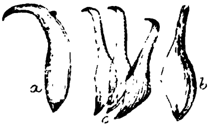
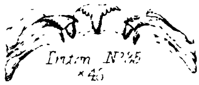
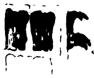


Philine No 23
×26


Triton No 25
×46



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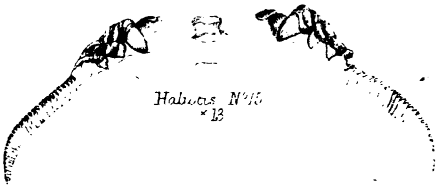
×48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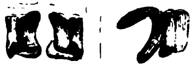
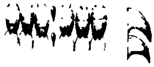
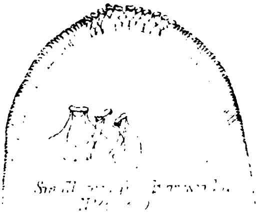


Patella (Acmaea) No 20
×60




Small [illegible]
No [illegible]

Molluscan palates

[illegible]

W. W. [illegible]

Gills exuding from end of back. Pink in colour, with dark blue and red spots; only found two specimens, three and four inches long respectively.

Palate, $\infty . 1 . \infty$, broad and short. Median very small, straight; laterals hooked; shape shown in side view.

Doris, No. 19.—Hab., low water on rocks; only found two or three specimens, one to two inches long; shape similar to *D. Johnstoni;* light orange colour.

Palate, a muscular band with a few irregularly-shaped concretions, can hardly be called teeth, distributed thereon without any apparent order.

The accompanying drawings of the palates of Mollusca are of those that I obtained (with one exception, Littorina) at Williamstown, at the head of Port Phillip Bay, some thirty miles from the open ocean.

They are the results of about three years' collecting, and, I believe, include all the species of palate-bearing Mollusca to be found in that particular locality (the statements as to rarity or otherwise are only applicable to it), but they are far from representing those of coast generally, as I have had no opportunity of collecting live shells elsewhere.

All the drawings are made direct from the microscope with the camera lucida. The nature of some of the palates of the smaller Turbinidæ is such that it is almost impossible, in mounting them in fluid, to prevent the edges from curling up, and I found great difficulty in seeing the second and third laterals of most of the limpets; they seem to be rudimentary, and hardly worthy the name of teeth.

The palates of the Turbinidæ are all very beautiful; some of them gorgeous when viewed with the polariscope and selenite plate, and they require a more skilful pencil than mine to show their beauty.

There are some Strombidæ, Volutidæ, Cypræidæ, and a beautiful "Paper Nautilus" (Argonauta) on our coasts, that I have not yet met with alive, but it will be seen that at the spot where my gatherings were made most of the families of Marine Gasteropoda were represented.

I have found two fresh-water shells (the most common is a Physa), and two or three others were forwarded to me by a friend, but a severe illness prevented me from mounting the palates, which were in course of preparation, for when I next was enabled to look at them they were destroyed. All their palates are of similar character to those of the Helicidæ.

I have seen only one land shell, that of a black snail (in our museum), but have not found any myself. The only localities in which they are likely to be met with here are the heads of gullies in the highest ranges, where the foliage of the fern-trees, &c., is dense enough to modify the summer heats.

My first impulse for research among the Mollusca was given by the late Mr. Hall asking me to obtain palates for him. A subsequent residence at the sea-side gave me the opportunity, which I availed myself of, and in my eagerness to make up a collection as soon as possible (my time for such work being limited) I regret I omitted to take any notes or make any drawings of the animals themselves. I am therefore unable to supply a great deficiency, as I cannot trust my memory for accuracy on this point. The shells I have drawn for myself in my diary, but have not forwarded copies, not deeming them to come under the head of "microscopical objects," though in absence of the shells themselves they would add greatly to the "natural history" value of the collection.

Some of the palates were kindly mounted by Mr. McIntire, and are in the Society's cabinet.

II.—*The Nerves of Capillary Vessels and their probable Action in Health and Disease.* By Dr. Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians; Physician to King's College Hospital.

(*Conclusion of a Paper commenced in the January Number of the Journal.*)

Part II.—Physiological and Pathological.

Considering the arrangement of nerve fibres in the organs of sense, no one would be likely to conclude that those distributed to capillary vessels were in any way concerned in special sensation. We know that many of the ultimate parts of organs which take part in sensation—which indeed constitute the active portion of the sensitive apparatus, are destitute of vessels altogether. These and other considerations render it certain that the nerve fibres of the capillaries are not nerves of special sense.

The capillaries are not provided with muscular fibres, nor is there any contractile tissue to be demonstrated in connection with them. Although it has been confidently asserted that the capillary walls consist of *protoplasm* (!), anyone who will be at the pains of examining actual capillaries, as, for example, those of the pia mater, will soon be convinced that this view is a mistake. It is, in fact, one of those conjectural anatomical observations which are in favour in these days. An hypothesis was advanced to account for the passage of blood corpuscles through the capillary walls, but before this hypothesis could be received it was necessary that some one should demonstrate the *protoplasmic character of the capillary walls.* Protoplasm was therefore conjecturally discovered in the capillaries by more than one microscopical philosopher.

The walls of capillaries may be well studied in the capillary vessels of the ciliary processes of the eye. The *membrane* of which they consist can be seen, and the bioplasts connected with it demonstrated in considerable number, and without difficulty. The membrane is transparent, slightly fibrous in the vessels of old animals, highly elastic, but destitute of any structure that can be regarded as *contractile.* The capillaries torn from the grey matter of the brain and those of the choroid plexus are also favourable for investigation. We may therefore, I think, dismiss the idea that these nerve fibres are motor, or are in any way concerned directly,—that is, by any influence exerted by them on the walls of the capillaries, in reducing the calibre of these minute vessels. If protoplasm could only be proved to exist in the walls of the capillaries, the presence and action of the nerve fibres would be accounted for. They would cause the protoplasmic substance to contract. But as the capillary parietes are not composed of protoplasm, but consist of a passive permeable membrane, such an explanation is inadmissible.

Next, I must ask the reader to consider with me whether it is probable that these nerves are connected with nutritive or secretory operations. Now, although many high authorities still hold to the opinion that nerves do act directly upon the nutritive process, many considerations render it at least doubtful whether the action of cells is directly influenced by nerve fibres in any case. Nutrition and growth are carried on at a rapid rate in living structures which are destitute of nerves, and at every period of life. In disease the most active nutritive changes occur in tissues which appear to be wanting in nerve fibres. For example, the formation of pus from epithelium and the formation of tubercle, can hardly be attributed to the influence exerted by nerves, seeing that the phenomena occur in parts to which nerve fibres do not reach. Nutrition and growth are most active in all living beings at a period of development anterior to that when nerves are formed. Secretion is very active in glands which receive a limited supply of nerves, and in those parts of glands to which very few nerves are distributed. Contrast, for example, the multitudes of fine nerve fibres distributed to the surface of a sensitive mucous membrane, with the few that can be traced around the uriniferous tube, or followed to the follicles of the sebaceous glands, or to those of the salivary glands. Pflüger's statements on this point have not been confirmed by other observers, and from my own observations I am convinced that if, as Pflüger asserts, nerve fibres are distributed to secreting cells, they are arranged in a manner very different from that represented by him in his drawings, and the character of the fibres is different. In those cases in which nerves can be followed near to secreting cells, it is more probable that their function is afferent as regards the nerve centre governing the nearest vessels, than that they are directly concerned in the actual secerning process. This I believe to be due rather to the properties and powers of the bioplasm of the cell, than to any mysterious, and at this time purely conjectural, influence of nerve force.

What, then, is the probable office of the nerve fibres which are so freely distributed to many of the capillary vessels? Upon many sensitive surfaces the fine nerve fibres running very close to the capillaries are indeed so numerous that we can hardly avoid coming to the conclusion that these very nerve fibres must be concerned in sensation. The capillaries of the mucous membrane of the frog's palate, for example, are surrounded by a complete network of very delicate nerve fibres, which lie immediately beneath the epithelium. In this situation there are no *papillæ*, nor is there any form of special sense organ to which nerves are distributed. By experiment we know that this surface is eminently sensitive, though there is no reason to think that it is concerned in the sense of taste. At any rate, upon the tongue of the frog are very highly elaborate nerve organs of a special structure, and having a very large number of nerve fibres distributed over a small space quite at the summit of the papilla.* If the frog possesses the sense of taste, the delicate papillæ above referred to are doubtless the organs concerned, and not the general surface of the palate.

But the nerve fibres near the capillary vessels may take part in general sensation. Doubtless it is through the agency of the beautiful little "tactile corpuscles" in the papillæ of the skin that we are able to distinguish those very slight differences of quality in fabrics when the tip of the finger is gently drawn across them. But if every tactile corpuscle were destroyed, we should still be able to *feel*, and the skin of the finger would still be differently acted upon by hot things and cold things. According to the same reasoning, we are justified in supposing that the nerves which I have shown are distributed to the capillaries of voluntary muscle, act as sensitive fibres, and are perhaps concerned in conveying to us the sensation by which we are enabled to form a conception of the exact degree of contraction which has been effected in the muscle under different circumstances. And in certain forms of disease in which there is no loss in the power of contracting the muscle, but in which the mind does not form an accurate idea of the degree of contraction which has been induced, it is probable that these nerve fibres, or the centres with which they are connected, are the particular parts of the nervous system which are involved. The cold feeling, the chill which precedes a cold or an attack of fever, results, I believe, from a change effected in these fibres distributed to the capillaries, caused probably by disturbance in the capillary circulation and the consequent phenomena induced immediately outside the minute vessels.

Few questions are of higher importance than the determination of the relationship of the nerves distributed to the capillaries to those ramifying in such great number on and amongst the muscular fibre cells of the small arteries which divide into branches from which the capillary vessels spring. I can adduce direct anatomical observations in favour of the view which I have been led to accept. In the frog I have succeeded in actually tracing nerve fibres from a ganglion to a nerve trunk, and from the trunk to the capillary vessels; and in the bladder of the frog I have been able to follow fine nerve fibres from the ganglion both to arteries and capillary vessels. It is surely justifiable to infer that a similar disposition exists in the higher vertebrata and in man. But such an inference is almost irresistible if we bear in mind the vast number of ganglia existing in the submucous areolar tissue of the intestinal canal and the course taken by the nerve fibres from these, and the fact of the great alteration frequently taking place in the vascular turgescence of the mucous membrane. But there are certain physiological experiments which have a very important bearing upon the question under discussion, and to these I will now direct attention.

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* See 'Phil. Trans.,' 1864.

Physiological Experiments.

Many years ago, when I possessed two living specimens of the Proteus, which I brought home from the cave at Adelsberg, I often observed the change which instantly took place when a bright ray of light was suddenly thrown upon the vessels of the exposed branchiæ. The little arteries of the gill suddenly contracted, and the entire volume of the vascular tuft was reduced by at least one-third. The circulation through the vessels was instantly retarded, the diameter of the capillaries was sensibly reduced, and the flow of the blood stream distinctly checked. The ray of light, I conclude, acted directly upon sensitive nerve fibres distributed around the capillaries, which carried an impression to the nerve centre, and through the vaso-motor nerves which were connected with this centre the muscular fibre cells of the artery were made to contract.

Another experiment which may be easily tried upon the web of the foot of the living frog is almost as conclusive, although it is open to the objection that the fibres which carry the impression to the nervous centre are not those of the capillary vessels, but the sensitive fibres distributed to the cutaneous surface. It must, however, be remembered that there are no special tactile organs in the part of the web experimented upon, and that the capillary vessels with their nerve fibres lie immediately beneath the thin cuticular covering of the web.

The experiment is performed as follows:—The foot of a young living frog is well arranged for observation and covered with thin glass, so that the circulation in the vessels can be well studied with a quarter of an inch object-glass. The illumination must be good. The tube of the microscope is then lengthened by eight or nine inches. By this proceeding we greatly augment the magnifying power. Next, a small artery is brought well into the field of the microscope, and carefully focussed. While the observer is looking intently at the artery the surface of the web near the spot under examination* is very gently touched with the point of a needle, which should have been already mounted in a handle made of a piece of light wood. Instantly the artery begins to contract, and in a few seconds its cavity is so narrowed that not a single blood corpuscle can pass. The contraction is not quite even, for the outlines of the vessel appear more or less wavy. After a few seconds an undulatory movement of the coats is noticed, and the artery gradually dilates to the same degree as before the irritation of the web.

* The experiment often succeeds when the most distant parts of the web are touched.

The results of these experiments, considered in connection with the facts discovered by careful anatomical investigation, prove, I think, conclusively the function of the fine nerves distributed to the capillary vessels, and indicate the precise manner in which they act. In many tissues and organs in which the circulation is readily disturbed by peripheral irritation the nerve fibres distributed to capillary vessels are exceedingly numerous.

Arrangement of Self-acting Mechanism by which the Supply of Blood to Tissues is regulated.

We are now in a position to consider the probable arrangement of the reflex nerve mechanism by which the flow of blood in the capillaries is regulated, and the equable distribution of nutrient fluid to the tissues outside the capillaries ensured in a state of health. We shall also see how this elaborate mechanism is rendered self-acting, self-regulating, and self-adjusting during the healthy state.

The nutritive operations of man and many vertebrata have to be carried on with comparatively little alteration under *very variable and continually changing external conditions.* A limited range of variation is permitted, but if the limits within which this self-adjusting apparatus acts perfectly, be overstepped in either direction, as not unfrequently happens under the very variable and artificial conditions to which civilized man and domestic animals are exposed, the range of the capacity for self-adjustment is exceeded, and a part of the mechanism is temporarily damaged or actually destroyed. Unfortunately repair can only be imperfectly carried out, and the mechanism restored to its normal state in cases in which the derangement is comparatively slight. So complex is the apparatus and so widely distributed are its interdependent parts that renovation after its destruction is complete is impossible, and I doubt if its re-formation in the adult has ever occurred, or is indeed possible.

In the diagram appended I have put together the facts I have ascertained by anatomical investigation, and have represented the several parts of which the self-regulating mechanism is, I believe, made up. I have also indicated what I believe to be their true relationship.*

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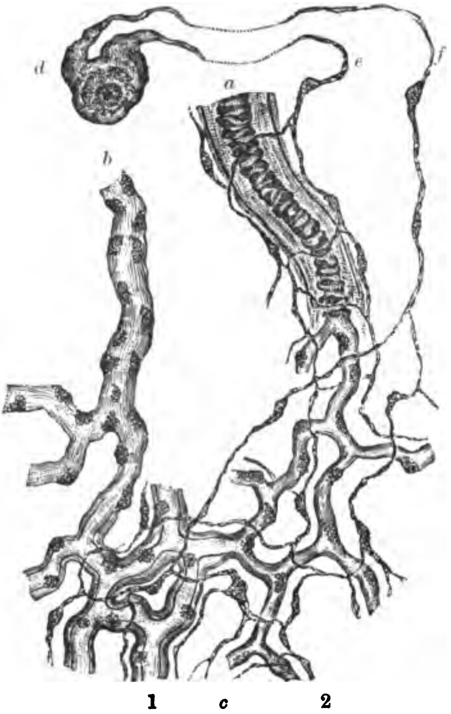
* The artery *a* is surrounded by muscular fibre cells which are supposed to be contracted, and the capillaries near the artery over the figure 2 are also represented as they would appear when very little blood was traversing them; while those to the left of the drawing, over figure 1, are as they would appear if distended with blood. It is obvious that in the first case an interval would exist between the nerve fibres and the capillary vessels, while in the latter, where the capillaries are distended, the external surface of their walls would be almost in contact with the nerve fibres and the particles of bioplasm connected with them. A dark line on each side of the artery indicates the diameter of the vessel when its walls are relaxed, and similar dark lines within the dilated portion of the capillary vessels, over figure 1, show the diameter of these vessels when the flow of blood through the artery is checked by the contraction of its muscular coat.

The arrangement represented we shall find will act as a self-regulating mechanism of the most perfect kind. Suppose for a moment a tissue is receiving more nutriment than it appropriates, the capillary nerve fibre will be at once disturbed, a change instantly transmitted to the nerve centre, and the afferent nerves distributed to the artery participating, will cause the muscular fibres to contract. The arterial tube is thus narrowed, and less blood consequently sent to the tissue. In an opposite state the phenomena would be



MECHANISM GOVERNING THE DISTRIBUTION OF BLOOD IN THE CAPILLARIES.

Diagram to show self-regulating mechanism connected with the minute arteries and capillaries. *a*, artery with muscular fibre cells; the dark lines show its diameter when dilated. *b*, small vein. *c*, capillary network. Over No. 1 the capillaries are dilated, and over No. 2 they are contracted. *d* is a ganglion cell with at least two sets of nerve fibres connected with it, one of which, *e*, divides and subdivides, giving off nerve fibres which are distributed to the artery *a*, while the other, *f*, is continuous with the plexus of nerve fibres ramifying close to the capillary vessels. Nerve fibres are also distributed to the small vein, *b*, but these are not represented in the drawing. The bioplasm of the vessels and nerve fibres is represented.

reversed—the arterial walls relaxed and the capillaries distended with blood. Or suppose that any noxious materials or living germs of any kind were making their way from without into the blood, it is obvious that if the nerve fibres distributed to the capillaries were healthy they would be instantly affected by the contact of the foreign body, and the vaso-motor nerves of the arteries would as instantly respond to the disturbance excited in the ganglion cell, and the contraction of the arteries would succeed. This would be followed by the narrowing of the capillary vessels, and a corresponding increase of thickness in their walls, a condition likely to prevent or at least to retard the ingress of particles to the blood. The foreign body thus kept from entering the current of the circulation might remain till it was destroyed or altered by the fluid, and thus rendered harmless, or, in the case of living matter, till its death occurred. In this way it may be that the capillary nerves of a person in perfect health protect him and are directly instrumental in preventing the access to his blood of substances which are with great difficulty changed, or which must infallibly produce disease if they find their way into the circulating fluid. The fibres distributed to the capillaries are probably those which are irritated or paralyzed by certain substances which after being absorbed by the blood transude through the delicate walls of the vessels, and are thus brought into contact with the nerve fibres just outside. These are the nerve fibres which are primarily affected in cases of sudden death which result from the presence of certain poisons, such as hydrocyanic acid, for example, and which are influenced in the production of those slower changes which arise from other poisonous matters introduced into the blood.

Influence of the Veins.

In regulating the flow of blood through the capillary vessels, it must not, however, be forgotten that the veins perform an important part. The muscular fibres are spread over irregularly and ramify in all directions upon the external surface of the vein, instead of simply encircling the tube, as in the case of the arteries. The thin muscular coat of the smaller veins is abundantly supplied with nerve fibres, which ramify upon and amongst the muscular fibres in greater number than in arteries. By the reduction of the calibre of the vein the circulation of the blood in the capillaries would be retarded; and as a given degree of contraction of the muscular fibre cells can be preserved for a certain period of time, it is clear that the veins share with the little arteries the office of regulating the flow of blood through the capillaries, and form an important part of that mechanism through the instrumentality of which the distribution of blood to the tissues and organs of man and the higher animals is constantly regulated and controlled. By the contraction of the muscular fibres of the small veins the lateral pressure of the blood upon the capillary walls may be altered from time to time, and the rapidity with which the blood traverses the capillaries caused to vary.

The smallest veins, like the capillaries, are destitute of muscular walls, but are remarkable for the great number of oval bioplasts connected with their coats and projecting into the interior of the vessel. In many of the smaller veins the united area of the bioplasts would considerably exceed that of the membranous portion of the wall of the vein. These bioplasts are the agents concerned in the absorption of nutrient constituents from the blood and their distribution in an altered form to the tissues. It is these bodies which, by removing and taking up certain of the substances dissolved in it, promote the onward flow of the blood. Thus the *vis-à-fronte* action, which has been so frequently referred to, may be accounted for and explained.

Action of the Nerve Fibres of the Capillary Vessels in Inflammation.

It is not possible that nerve fibres lying so very close to the capillary walls should be uninfluenced when the volume of blood in the capillary is increased, as in congestion. By the long-continued stretching or pressure to which these delicate nerve fibres would be subjected, partial paralysis would be induced.

In inflammations and in the long-continued feverish condition, the growth of bioplasm external to the vessels and resulting from the multiplication of bioplasts which had passed through the capillary walls in the stage of congestion, must seriously impair the action of these nerve fibres; and there is good reason to think that some of the phenomena that ensue are the direct consequence of the nerve disturbance. Let us therefore consider very briefly the changes occurring in one of the slightest and most familiar forms of inflammation—a common flea-bite.

The tissues which are perforated by the cutting instrument of the flea as it penetrates, would be, first, the epithelium; next, that modified connective tissue with its "corpuscles" (masses of bioplasm) of which the papillæ and the most superficial layer of the true skin consist. Embedded in this connective tissue are certain vessels and nerves. The following tissues would be more or less damaged by the passage of any sharp instrument through the different layers of the skin,—cuticle, connective tissue, the capillary vessels (lymphatics in some instances), and nerves; and these are the only tissues which can be affected under the circumstances alluded to. Now the capillaries, as we know, receive blood from the arteries; and the calibre of the latter vessels, and therefore the quantity of blood traversing these and the capillaries, is governed by nerves.

Now it will be noticed in a flea-bite or other inflammation of skin that there is a certain blush which is very deep in colour immediately around the wound, but extends a considerable distance beyond the point actually injured. This inflammatory blush too, as can be easily proved, may be completely removed by pressure. If I press the skin, the tint of the inflamed portion will become of precisely the same hue as that of the surrounding healthy skin; the whole is perfectly pale. When I withdraw the pressure, and the blood is permitted to return, the capillaries of the normal skin are distended to precisely the same calibre as before, and the dilated vessels in the seat of inflammation are again dilated, and to the same degree as before the blood was pressed from them. This simple experiment proves not only that there exists some mechanism by which the calibre of the vessels may be not only increased and reduced so as to allow a larger or smaller quantity of blood to flow through the capillaries in a given time, but that the increased quantity of blood which flows through these tubes in inflammation is fixed and regulated by the degree of contraction of the muscular fibre cells of the small arteries. This can be determined only through the instrumentality of nerves and nerve centres in which a certain temporary change has been established and is maintained for a time. Such an arrangement as that described in page 59 would enable us to explain the facts above referred to.

In the early stages of inflammation probably more blood actually flows through the capillaries of the inflamed part than through those of the healthy tissues; but after a time, as is well known, the circulation becomes slower, the capillaries are distended, and at last the blood ceases to flow. The self-regulating mechanism fails to act, and, unless relief speedily follows, may be destroyed.

Inflammation, as it occurs in the tissues of the higher animals, is an exceedingly complex process, in which vessels, nerves, and many other structures take part. But other changes are occurring during inflammation which produce an influence upon the nerve fibres outside the capillary vessels though less directly. If the inflammatory change is not quickly succeeded by a return to the normal state, partial or complete paralysis of the little arteries results; the capillaries necessarily become distended, and their walls so attenuated, that the passage of fluids through them from the blood is very much facilitated. There is increased *exudation*. Now the exudation that escapes is not simply a clear fluid from which particles are deposited after the manner of crystals, and aggregated so as to form "cells," but at the time it is poured out there are suspended in it numerous living particles. These particles are found in considerable number, and are probably composed of matter like that which enters into the composition of the white-blood corpuscle. Moreover, the small particles detached from the white-blood corpuscles make their way through, and divide, and subdivide, and give rise in a short time to multitudes of minute bioplasts which can only be distinguished by the aid of very high magnifying powers. This was referred to in a paper which was presented to the Microscopical Society in 1863, and published in the 'Transactions.' No doubt then, in many cases where the capillary vessels are distended, living particles of bioplasm pass through. These being surrounded with nutrient matter, grow, and divide and subdivide very freely. In this way are produced the multitudes of bioplasts commonly found immediately surrounding the capillary vessels in the earlier periods of inflammation of complex textures. Wherever fluid in which are suspended living particles stagnates, the conditions are favourable for the absorption by the living bioplasts of an increased quantity of nutrient material. Not only may this increased absorption of nutrient material occur outside capillary vessels, but it may proceed in the substance of a clot. The white-blood corpuscles entangled in the clot have been known to multiply to such an extent as to give rise to the formation of multitudes of bioplasts which resemble a drop of pus. Pus has been formed in a clot of fibrine. It is possible that pus may be formed in the same way in a clot of blood.

In this way I would venture to explain also the changes which ensue in the serous or fibrinous exudation which occurs in the inflammation of certain special tissues. As is well known, serous membranes are subject to what is known as adhesive inflammation, which is supposed to be distinct from the suppurative. But pus may be formed upon the surface of a serous membrane. When we consider the structure of the serous membrane, and bear in mind how exceedingly thin it is, and how close the vessels are to its free surface, it is easy to conceive that minute particles of bioplasm would pass through, collect upon the free surface, grow and multiply, and give rise to the enormous quantity of fibrinous material frequently seen, for example, upon the surface of the pleura and pericardium in inflammation. In this way is the "recent lymph" probably produced. And we know that pus may be formed if the process goes on. As nutrition increases, the little bioplasts multiply exceedingly; the "lymph" is found to consist almost entirely of bioplasts, which take up the fibrinous matter, and at last "pus," which, as I have shown, consists of multitudes of rapidly-growing living bioplasts, results.

It is not possible that such changes as those described could take place just outside capillary vessels without seriously implicating the nerves themselves, leading thereby to disturbance of the nerve centres, and by reflex action to the extensive and widely-distributed nervous derangement which usually accompanies severe inflammations and general fevers. In both classes of diseases the characteristic general disturbance is due entirely to changes in the capillary vessels and in the nerves of the capillary vessels which I described in the first part of this paper, published in the January number of the Journal.

Alteration of Nerve Fibres of Capillaries in Chronic Disease.

In many forms of chronic disease the nerve fibres distributed to the capillary vessels, as well as those ramifying upon the small arteries and veins, degenerate and are at length destroyed. So that the variation in vascular tension and blood distribution which occur in the healthy state through the agency of these nerves cannot take place. If therefore in consequence of the body being exposed to very adverse conditions the circulation be much disturbed, the derangement cannot be compensated or the injury repaired. It too often happens that the damage is increased until sufficiently great to render a fatal result inevitable.

In fatty degeneration of the capillary vessels and small arteries, the fine nerve fibres are also involved, and the mechanism for regulating the flow of blood through the small vessels is gradually impaired. In some cases the nerve fibres and the muscular fibres of the arterial coats have so completely degenerated that the distribution of blood is greatly impeded. The outline of the vessels becomes uneven, their coats thicker in some places than in others, and the lining membrane rough. The elasticity of the vascular walls is much impaired, and in not a few instances the tube is actually rigid. The nutrition of every organ in the body suffers partly from changes which affect the circulation of the blood, and in part from the altered composition of the blood itself, which is gradually induced by the deranged circulation occurring in the blood-forming and blood-changing organs.

The condition of *health* is dependent upon the integrity of the nerve fibres distributed to the capillary vessels. It is therefore of the utmost importance to do all we can to keep these delicate nerve fibres in an active state. By exercise, cold bathing, and external rubbing, that amount of activity of the nerve fibres and change in the capillary circulation, is ensured at least once in the twenty-four hours that is required in order that the nerve mechanism which has been described may be exercised and its activity preserved. These nerve fibres are, as it were, the sentinels which give the earliest information of the dangerous proximity to the vessels of matters the entrance of which into the blood might occasion serious disease or cause death. So long as these sentinels are active and in thorough good working order, warning is given in time for the performance of acts which prevent the ingress of deleterious substances; but if they act sluggishly, or if the excitability of these nerve fibres is numbed by certain agents, time is allowed for the poison just outside the vessels to make its way into the blood; while if they are paralyzed, even temporarily, the state of things is far worse. No watch is kept, and the organism may be destroyed by an invading poison that under other circumstances might have been effectually excluded.

III.—*Remarks on the Nomenclature of Achromatic Objectives for the Compound Microscope.*

By Dr. J. J. Woodward, U. S. Army.

For some years past, while most of the Continental opticians have continued to give arbitrary designations to their achromatic object-glasses, such as No. 1, No. 2, &c., or System A, System B, &c., the English and American manufacturers, affecting a higher degree of accuracy, have undertaken to name the objectives they construct by their real or supposed agreement in magnifying power, with single lenses of specified focal lengths. We hear accordingly of inch, half-inch, and quarter-inch objectives, &c., by which we are expected to understand combinations agreeing in magnifying power with single convex lenses of the focal lengths named.

At first sight nothing could appear simpler or more exact than such a nomenclature; nevertheless recent articles in the journals would seem to indicate that the general plan is capable of considerable modification in its practical application, and that grave misunderstandings have hence arisen.

Under these circumstances, it appears desirable to give some account of the principles involved, and of the practical difficulties to be considered in their application, particularly as the microscopical text-books contain little or no information on the subject. In fact, the only scientific discussion of the matter with which I am acquainted is the paper of Mr. Charles R. Cross, "On the Focal Length of Microscopic Objectives."* This paper gives a reasonable formula for the approximate computation of equivalent focal lengths, and furnishes some other valuable information, but does not discuss all the points at issue. I shall have occasion to refer more than once to this excellent paper, which the reader would do well to examine in connection with the following remarks.

We learn from the elementary treatises on optics that when an object is placed in front of a single convex lens at a distance somewhat greater than its focus for parallel rays, a real image is formed on the other side of the lens, which may be received on a screen. This image will be larger, and formed at a point more distant from the lens, the nearer the object approaches to the focus for parallel rays; and two equations are given which express the relationship of the distances to each other, and to the magnifying power, *viz.* $\frac{1}{f} = \frac{1}{p} + \frac{1}{p'}$, and $\frac{p'}{p} = m$, in which f is the length of the focus for parallel rays, p the distance of the lens from the object, p' its distance from the image, and m the true magnifying power, that is, the size of the image divided by the size of the object; p and p' are termed the conjugate foci, and are variable quantities; f is termed the principal focus, and has an unchangeable value for each single lens.

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* 'Journal of the Franklin Institute,' June, 1870, p. 401.

If now we combine the above equations, representing $p + p'$ or the sum of the conjugate foci by l, we may deduce the formula $f = \frac{m\,l}{(m+1)^2}$, which represents in the case of any single convex lens the relationship existing between the length of the principal focus, the magnifying power, and the distance from the object to the screen. This formula, which I think rather more convenient than that of Mr. Cross, differs from it only in using $m =$ the magnifying power, instead of $n =$ the reciprocal of the magnifying power; it may be deduced from his by substituting for n its value $\frac{1}{m}$ and reducing, or it may be derived directly from the primitive equations. In either shape the formula yields the same numerical results, and if for any single convex lens m and l are given, the accuracy of the value of f resulting will be limited only by the degree of precision with which m and l have been measured.

If now there were any such actual equivalence between achromatic objectives and single lenses as the nomenclature assumes, it would only be necessary to set up the objective to be rated, in such a manner that the image of a micrometer should be focussed upon a white screen, using of course no eye-piece, to measure the distance from the micrometer to the screen, to determine the magnifying power by measuring the image of the micrometer, and substituting these values of l and m in the working formula to calculate the value of f. Unfortunately, however, if with any compound objective we repeat this operation several times, merely varying the distances, we obtain as many different values for f as there are distances used, instead of obtaining but one value for all distances as we do with a single lens.

Mr. Cross* has already pointed out this circumstance, which results from the fact that the modern achromatic objective has considerable thickness from its anterior to its posterior surfaces, and that it has, properly speaking, no true optical centre. He gives two examples, in one of which a change of 5·24 inches in distance corresponded to a change of ·0067 inch in calculated focal length; in the other a change of 6·10 inches in distance corresponded to a change of ·0029 inch in calculated focal length, the objectives used in the experiment being uncorrected ¼ths, so called. If, however, Mr. Cross had used for this purpose lower powers, or had made greater variations in the distances employed, he would have found much greater discrepancies. For example, by measuring the magnifying power first at 25 and then at 50 inches, and deducing the value of *f* by the formula of Mr. Cross, I obtained in two cases the following equivalent focal lengths:—For a so-called 1¼ inch, at 50 inches distance, 1·2187 inch; at 25 inches distance, 1·2468 inch; difference, ·0271 inch. For a so-called ⅕th, at 50 inches distance, ·1982 inch; at 25 inches distance, ·1893 inch; difference, ·0089 inch.

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* *Loc. cit.*, p. 409.

Moreover, since the achromatic objectives of different makers are constructed on different series of curves, and the component lenses placed at different distances apart, it will be found that if two achromatic objectives magnify the same at any given distance, they will no longer do so if the distance is materially changed.

Hence I am compelled to agree fully with the observations of Mr. Cross[*] that the nominal focal length assigned to an achromatic objective can only serve in any case as "a general appellation serving to group together objectives of *approximately* the same magnifying power," and must conclude, therefore, that the English and American nomenclature possesses no real claim to strict scientific accuracy, and especially that the comparison made by some with the case of the celestial telescope is not valid.

But besides the inevitable inaccuracy resulting from this source, there are in the case of the higher powers of modern makers two other sources of much more considerable error. The first of these involves the case of all those objectives which are provided with a screw-collar to correct for thickness of cover; the second involves the case of objectives with two fronts, one for wet and the other for dry, or those with but one front which can be used wet or dry by merely changing the correction given by the screw-collar.

The correction for thickness of cover is made, as is well known, by changing the distance between the front combination of the triplet and the posterior two combinations. As a consequence, the magnifying power of the objective at any given distance, or with any given eye-piece, is least when the objective is corrected for uncovered, and greatest when it is corrected for the thickest cover through which it will work. The ratio of this change is very different in different objectives. I select a few from my note-book in illustration, purposely omitting to name the makers :—

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[*] *Loc. cit.*, p. 401.

	Magnifying power at uncovered.	Magnifying power at covered.
No. 1	200	225
No. 2	250	275
No. 3	300	333
No. 4	350	500
No. 5	570	630
No. 6	900	1100
No. 7	975	1180
No. 8	890	1250

These values were obtained by throwing the image of a micrometer on a cardboard screen, using the objective without an eye-piece, and the distance from micrometer to screen in each case remained the same, the screw-collar and the focal adjustment of the objective being modified. Of course, for all intermediate positions of the screw-collar, intermediate values result. The distance used was 48 inches in some of the cases, 50 in the others. With shorter distances, the amount of the difference is diminished, but its ratio to the magnifying power at uncovered is not materially changed, as anyone can convince himself by comparing a stage micrometer, as seen by any corrected objective, with an eye-piece micrometer, first at uncovered and then with the full correction for cover.

Now it is evident that even if, by the formula of Mr. Cross, or otherwise, we could obtain accurate equivalent focal lengths for any one position of the cover correction, the result would not be true for any other position of the cover correction.

We are told by a recent writer that the practice of the opticians is to name the combination at its performance uncovered, that is, at precisely the adjustment least used. If, however, scientific accuracy in the matter is desirable for any purpose whatever, it is evident from the above that at least the maximum and minimum should be furnished by the maker.

It is also evident that the considerations here offered, aside from their bearing on the nomenclature of objectives, have a high practical value to all those who attempt to make micrometric measurements with modern high-power objectives; for the practice recommended in the text-books, and too generally pursued, is to give values to an eye-piece micrometer by comparing it with a stage micrometer at a fixed position of the draw-tube, and to use these values in subsequent measurements. Now as these values vary considerably with the cover correction, it is to be feared that the majority of the measurements made with high powers during the last twenty years are sadly inaccurate.

The difficulties in the way of a nomenclature based upon equivalent focal lengths have been still further increased since the introduction of immersion objectives. The compound objective is usually furnished with two fronts, one for wet and one for dry use. Of these the wet usually gives the greatest magnifying power at any given distance. I have also measured an objective which, when corrected for the thickest cover through which it will work dry, is just corrected for uncovered wet, and by approximating the posterior pair of combinations still nearer to the anterior, corrects for cover wet, thus increasing the magnifying power when the objective is in use wet precisely as if two fronts were used.

The difference in magnifying power resulting from the modifications given to make the objective perform wet is quite considerable, as may be seen by the following extracts from my note-book.

	Magnifying Power.			
	Dry.		Wet.	
	Uncovered.	Covered.	Uncovered.	Covered.
No. 1	225	250	250	275
No. 2	425	490	450	500
No. 3	700	900	900	1000
No. 4	770	910	900	1100
No. 5	790	930	975	1180

Of these objectives No. 1 had but one front, the correction for wet being made by the cover correction; the others had two separate fronts, one for wet, the other for dry.

Now it is just in connection with these complex objectives, with double fronts or other devices to correct for wet and dry, that the greatest diversity of nomenclature exists. In one quarter it is claimed that the system should have two names, one based on its magnifying power at uncovered wet, the other on its magnifying power at uncovered dry; in other quarters the practice has been to give the system but a single name, derived from the magnifying power at uncovered dry alone. It is evident, however, that neither of these plans has any pretension to scientific accuracy, and that if the makers will not give at least the maximum and minimum for both dry and wet, the purchaser must learn to measure for himself.

A similar remark applies to the angle of aperture as stated by the makers, for each objective sold. As a rule they give the greatest angle attainable by the combination, which is generally—not always—the angle at the correction for thickest cover, their usual practice in this case being the reverse of their usual practice with regard to magnifying power. Now I have seen objectives with an angle of 170° and upwards at full cover correction, which did not exceed 140°

at uncovered. It is evident, therefore, that the maker should furnish with each glass both the maximum and minimum angle, or the microscopist must measure for himself.

After a full consideration of all the circumstances, I am disposed to think that the best interests of both makers and purchasers would be consulted if the present nomenclature were abandoned altogether, and objectives named instead by their precise magnifying power without eye-piece at some selected distance. It would be well if all the makers could be brought to agree on some fixed distance; but until we obtain this happy uniformity, which perhaps is not to be anticipated, it is only necessary for each maker to state the distance he selects. By this plan objectives without correction for cover would be named by one number, objectives with correction by two, and those with two or more fronts or backs by two or more pairs of numbers.

Thus we should have objectives without cover corrections named precisely 2, 3, 4, 5, 6, 7, 8, 9, and so on up to 100 or more, the number indicating the exact magnifying power, say at 12½ inches from micrometer to screen. Objectives with cover corrections would be named 30 to 40, 35 to 46, 75 to 89, 125 to 140, &c., the numbers representing the minimum and maximum magnifying powers at the selected distance. Objectives with wet and dry fronts would require a separate name for each; thus 78 to 95 dry, 98 to 130 wet, &c.

By this plan the real magnifying power of the glass and its limits of variation would be accurately stated, whereas at present even those makers who are most careful about their nomenclature do not hesitate to call a glass an ⅛th provided its power at uncovered approximates that of a single lens of ⅛th of an inch focus more nearly than it does a ⅙th or a $\frac{1}{10}$th. Hence glasses which differ materially in magnifying power at uncovered, receive the same name, and the changes in power produced by the cover correction are invariably ignored.

If the plan I here recommend be adopted, the precise distance from micrometer to screen which may be chosen does not appear to me to be of very great importance. Many persons would, I suppose, prefer 10 inches to 12½. I have selected the latter number because many of our larger stands have tubes too long to permit the convenient measurement of low powers with eye-piece and stage micrometers, in the manner I shall presently describe, if the distance be taken at 10 inches, whereas on most stands, with the help of the draw-tube, 12½ inches can be used conveniently with all powers from the 3-inch upwards. I do not, however, insist on any particular distance, but only that the distance selected shall be stated in each case until some uniform plan shall be generally agreed upon.

In this connection I may add that the actual plan of the Continental opticians is also unsatisfactory. In the matter of angle of aperture, when they give it at all, they also, as a rule, give only the maximum. In the matter of magnifying power, when they give any information, they attempt, as a rule, to give the magnifying power of the objective with each eye-piece as actually looked through when in use. But as the magnifying power of the combination under these circumstances involves the distance of distinct vision for each observer, it is evident that the figures thus furnished cannot have any practical value.

Pending the adoption of some such system as I have suggested above, it will be necessary for the microscopist to measure for himself the magnifying power of the objectives he uses, whenever he desires to be possessed of the real information concealed behind all the several systems of nomenclature at present in use. Mr. Cross, in his paper, gives two modes of doing this. I myself have been in the habit of using a dark room, and throwing the image of the micrometer on a white screen, by the direct rays of the sun. Very nearly equal accuracy may be attained, however, by any microscopist who possesses an ordinary glass eye-piece micrometer and a stage micrometer. The glass eye-piece micrometer, it will be recollected, is slipped through the eye-piece in such a manner as to be just in the focus of its eye-glass, that is, in the plane which is occupied by the image formed by the objective when it is seen most distinctly. If now the field-glass of such an eye-piece be removed, and the stage micrometer carefully brought into focus, a comparison of the divisions of the eye-piece and stage micrometers will give the magnifying power of the objective alone at the distance actually existing between the two micrometers. Thus, for example, if the stage micrometer is in $\frac{1}{1000}$ths and the eye-piece micrometer in $\frac{1}{200}$ths of an inch, five times the number of eye-piece divisions corresponding to one division of the stage micrometer will be the magnifying power of the objective at the cover correction employed, for the distance selected. We may then apply the following simple rule, derived from the formula of Mr. Cross.

Multiply the distance between the two micrometers in inches and decimals, by the magnifying power, and divide by the square of the magnifying power plus one; the result will be the equivalent focal length (for the given conditions) in decimals of an inch.

It will, moreover, be found in practice that for powers equivalent to those of a $\frac{1}{4}$th or shorter focal lengths a still simpler rule may be adopted, and that the distance between the two micrometers, divided by the magnifying power, will give very nearly the same results as are obtained by the more complex rule, but this of course is not true for lower powers.

It must, however, be constantly borne in mind, that the results obtained in any case are true only for the cover correction and distance used.

For the convenience of those who may undertake such comparisons, I append a Table in which the real magnifying powers of single convex lenses are given for three different distances. In the treatises on optics, the magnifying powers of single lenses are sometimes stated at some given distance from the lens to the screen, but I know of no table which shows their powers at given distances between image and object.

This Table is calculated by substituting the numerical values of f and l in the equation $f = \frac{m\,l}{(m+1)^2}$, when m = the magnifying power remains as the only unknown quantity, and is easily computed. I have carried out the values of m to two decimal places only; but in practice the nearest whole number will be found sufficiently accurate.

TABLE OF THE MAGNIFYING POWERS OF SINGLE CONVEX LENSES.

Focal length for parallel rays.	Magnifying power at 12½, 25, and 50 inches distance from micrometer to screen.		
	12½ inches.	25 inches.	50 inches.
3 inches	1·50	6·17	14·59
2 "	4·00	10·40	22·95
1½ inch	6·17	14·59	31·30
1 "	10·40	22·95	47·99
⅔rds of an inch	16·69	35·47	72·98
$\frac{4}{10}$ths "	29·21	60·48	122·99
¼th "	47·99	97·98	197·99
⅕th "	60·48	122·99	247·99
⅙th "	72·98	147·99	297·99
⅛th "	97·98	197·99	397·99
$\frac{1}{10}$th "	122·99	247·99	497·99
$\frac{1}{12}$th "	147·99	297·99	597·99
$\frac{1}{15}$th "	185·49	372·99	747·99
$\frac{1}{16}$th "	197·99	397·99	797·99
$\frac{1}{18}$th "	222·99	447·99	897·99
$\frac{1}{20}$th "	247·99	497·99	997·99
$\frac{1}{25}$th "	310·49	622·99	1247·99
$\frac{1}{50}$th "	622·99	1247·99	2497·99

Note.—Since writing the foregoing article, I have read with pleasure a paper on the same subject by Dr. R. H. Ward.* This paper contains much valuable matter, and should be read by all who are interested in this subject. I learn from it for the first time that the method of determining the magnifying power of objectives by removing the field-glass of the eye-piece and using eye-piece and stage micrometer as described above, has already been used by Dr. J. J. Higgins, of New York,[*] to whom I hasten to give the credit due.

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* "Remarks on Uniformity of Nomenclature in regard to Microscopical Objectives and Oculars," 'American Naturalist,' March, 1872, p. 136.

I am very glad to find Dr. Ward indicates the feasibility of the substitution of magnifying powers at a fixed distance as names for objectives instead of alleged equivalent focal lengths. I must, however, differ from him when he recommends that the distance should be measured from some part of the objective to the screen, instead of from the micrometer to the screen; for however desirable this may be made to appear, it is not feasible. Nor can I agree with him in regarding 10 inches distance as having anything special to recommend it. To the question he asks at the conclusion of his paper, "at what point of the screw-collar adjustment shall the objective be placed for rating its angular aperture and amplifying power?" I reply without hesitation, that for each purpose accuracy demands that the maximum should be given as well as the minimum; that the maker should state not merely one limit, but both.

Dr. Ward's paper also contains some interesting suggestions on the subject of the nomenclature of eye-pieces, a matter which will, however, I think, particularly in the case of the ordinary eye-piece, require further discussion. I agree fully with Doctor Ward that eye-pieces should be named by their magnifying power: but the question at once arises, how shall this be accurately measured? To this subject I may recur at some future time.—*Silliman's Journal*, June.

IV.—*Our Present Medical and Students' Microscopes.*

By R. H. Ward, M.D.

Those who use the microscope as an elegant and costly luxury will, of course, be guided in so doing by their general ideas of taste, economy, &c.; the few who use the instrument as medical experts, or original investigators in science, will, at the same time, by years of practice, grow into the use and the possession of an instrument suited to their wants; but a larger class are those who use the instrument as an incidental though frequent aid in their daily work in various sciences or professions, who reasonably desire the simplest instruments consistent with real usefulness, and who, however eminent in other specialities, are often unfamiliar with the styles and prices of the various makers, and at a loss to know what available resources would best supply their wants. The following Tables are designed to be of use to buyers of microscopes, of the latter class, and to persons who desire information in a concise and convenient form, in regard to the progress thus far made in this department of microscopy. While the styles and prices will be subject to endless variation, the statements made will be sufficiently accurate to form a basis for selection and correspondence for a considerable time.

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[*] 'American Naturalist,' Dec. 1870, p. 628.

Only standard and available styles are given, by reliable American makers, and no such makers have been intentionally omitted. The selection of instruments and apparatus of the different makers is entirely the responsibility of the writer, and not of the makers. He has selected such sets, at the makers' prices, as he has been accustomed to recommend to students and others for the general purposes of students' microscopes.

The Table of Objectives is designed to be complete in regard to American lenses, and to present for comparison, and for the convenience of those who desire a variety of lenses by different makers, such European items as could be obtained at the time of writing. It is believed that the Table will be interesting to others than buyers, as showing the prevailing powers, prices, angular apertures, &c., in different parts of the world. Notwithstanding the general impression that the prices of American objectives are unreasonably high, it will be seen that they compare favourably with those of the most celebrated English makers. Most opticians prepare, very cheaply, mounted objectives of small angle, and usually second quality, which answer a good purpose where economy is imperative and difficult work is not required. Such lenses are not usually priced and sold separately by the American makers, and are therefore not included in their columns in the Table of Objectives.* They can be obtained, however, at prices proportioned to their angles and quality. The prices and angular apertures given in the Table are generally, though not in every case, official and up to date; but a reasonable allowance must be made for practical variation from the standard angles and powers. There is understood to be a screw-collar adjustment for all objectives of over 50° angular aperture, unless otherwise stated, though, from want of sufficiently distinct description by their makers, this rule cannot be applied to some of the English lenses of medium power and small angle. To persons without great experience in microscopy, the best "test" of an objective is the respectability of its maker.

The binocular microscope, though not especially applicable to medical microscopy, is of such general usefulness that it should be preferred in all cases where its price is not a positive objection. Binoculars are furnished, when ordered, even by those makers who do not manufacture them.

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* This remark would also apply to a few of the English opticians.

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES.

a. See Table of Students' Microscopes.
b. C. Baker, 243 and 244, High Holborn, London.
c. R. & J. Beck, 31, Cornhill, London, E.C.
d. John Browning, 63, Strand, London, W.C.
e. Charles Collins, 157c, Great Portland Street, Portland Place, London, W.
f. Henry Crouch, 51, London Wall, London, E.C.
g. J. B. Dancer, 43, Cross Street, Manchester.
h. Horne & Thornthwaite, 122 and 123, Newgate Street, London, E.C.
i. James How, 2, Foster Lane, London, E.C.
j. Murray & Heath, 69, Jermyn Street, London, S.W.
k. M. Pillischer, 88, New Bond Street, London, W.
l. Powell & Lealand, 170, Euston Road, London.
m. Thomas Ross, 7, Wigmore Street, Cavendish Square, London, W.
n. J. H. Steward, 406, Strand, London, W.C.
o. James Swift, 43, University Street, Tottenham Court Road, London, W.C.
p. Edmund Wheeler, 48, Tollington Road, Holloway, London, N.
q. E. Hartnack & Co., 21, Place Dauphine, Paris, France.
r. E. Gundlach, Optical Institute, Charlottenberg, near Berlin, Prussia, Germany.
s. G. & S. Merz, Optical Institute, Munich, Bavaria, Germany.
t. R. Wasserlein, 27, Schutzen Street, Berlin, Prussia, Germany.
u. Carl Zeiss, Jena, Germany.

Nominal focus in inches.	Angular aperture in degrees.	a. Grunow.[a] Price in dollars.	a. Miller. Price in dollars.	a. Queen. Price in dollars.	a. Spencer. Price in dollars.	a. Tolles.[b] Price in dollars.	a. Wales.[c] Price in dollars.	a. Zentmayer. Price in dollars.	b. Baker. Price in pounds and shillings.	c. Beck. Price in pounds and shillings.	d. Browning. Price in pounds and shillings.	e. Collins. Price in pounds and shillings.	f. Crouch. Price in pounds and shillings.	g. Dancer. Price in pounds and shillings.	h. Horne & Thornthwaite. Price in pounds and shillings.	i. How. Price in pounds and shillings.	j. Murray and Heath. Price in pounds and shillings.	k. Pillischer. Price in pounds and shillings.	l. Powell and Lealand.[d] Price in pounds and shillings.	m. Ross. Price in pounds and shillings.	n. Steward. Price in pounds and shillings.	o. Swift. Price in pounds and shillings.	p. Wheeler. Price in pounds and shillings.	q. Hartnack.[e] Price in francs.	r. Gundlach. Price in thalers.	s. Merz. Price in thalers.	t. Wasserlein. Price in thalers.	u. Zeiss. Price in thalers.
5	..	..	..	..	..	..	..	16½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	7	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..
4	8	..	..	..	..	18f	..	16½	1 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	9	..	..	..	..	..	12	..	..	..	..	..	1 0	..	..	..	1 8½	1 10	1 10	1 10	1 10	1 0	..	..	..	..	..	..
	10	..	..	..	35g	35g	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	12	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
3½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..	..	..	..	..	..
3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 13	..	..	..	..	..	..	..	..	..	..	..	..	..
	6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 1	..	..	..	..	..	..	..	..	..	..	..	..
	8	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..	0 15	..	..	..	..	..	..	..	..	..	..	..
	10	..	13½	..	..	..	..	..	1 15	..	2 0	1 10	..	1 0	..	..	..	..	..	..	1 5	1 0	1 10	..	..	..	..	..
	12	18	18	..	..	..	15	..	..	3 10	..	2 0	1 10	2 5	..	..	2 15	1 10	2 15	3 0	2 10	1 10	2 2	..	..	..	..	..
2	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	15	..	..	..	..
	10	..	..	..	..	20	..	..	..	1 10	0 18	..	..	..	..	1 1	0 18	..	..	..	..	..	..	..	..	..	..	..
	12	..	13½	..	15	..	..	..	1 10	..	..	..	1 0	1 0	..	..	1 5	1 10	..	..	1 5	..	1 10	..	..	..	..	..
	13	..	..	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..	..	1 0	..	..	..	..	..	..
	14	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 15	..	..	..	..	..	..	..	..	..

	15	..	18	..	..	..	..	..	1 17½	..	..	2 0	1 10	2 5	..	2 10	2 15	2 10	..	3 0	2 10	1 10	2 2	..	..	..	..	..
	16	..	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	18	..	..	..	..	23	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	20	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	27	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1¼	12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 1	..	..	..	..	..	..	..	..	..	..	..	..
	13	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 5	..	..	..	..	..	..	..
	15	12	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	..
	16	..	..	..	..	..	..	..	..	..	..	..	..	1 10[h]	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	17	..	..	..	..	28[i]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	20	..	22	..	..	..	..	..	1 17½	..	2 5	..	1 10	2 10[h]	..	2 15	2 17½	..	3 0	3 0	2 15	1 10	2 10	..	..	..	..	..
	22	18	..	..	..	..	17	15	..	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..
	23	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	37	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	20	..	..	..	5
	15	..	..	..	..	..	..	..	1 10	..	0 18	..	..	..	..	1 1	1 0	1 10	..	2 0	1 5	..	..	..	..	..	..	..
	16	..	13½	..	..	..	..	..	..	..	..	..	1 0	1 10	..	..	..	..	..	..	..	..	1 10	..	..	..	..	..
	17	..	..	..	..	20	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	18	..	..	12	..	..	..	..	..	..	..	1 10	..	..	..	..	..	..	..	..	..	1 0	..	..	4	..	..	..
	20	..	..	..	20	..	..	..	..	..	..	..	..	2 10[f]	..	..	1 15	..	..	..	..	..	..	..	..	8	..	..
	22	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	23	..	..	..	..	..	..	..	1 17½	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	25	20	22	..	..	23	..	..	..	..	2 10	2 0	1 10	2 10	..	3 3	3 0	2 10	..	3 10	3 0	1 10	2 10	..	..	..	..	..
	27	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..
	30	20	..	..	30	30	..	..	2 2	..	..	..	..	..	..	..	..	..	3 3	..	..	..	..	..	..	..	..	..
	35	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 0	..	..	..	..	..	..
	40	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	45	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
8–10	20	..	..	12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2	..
	32	..	..	..	..	..	..	18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
3–4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	25	..	..	..	..
	40	..	..	..	..	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
2–3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..	..	..	..	..	..	..	7
	22	..	..	15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	25	15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	28	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..
	30	..	22	..	..	..	20	..	..	..	..	2 10	1 10	..	..	..	..	..	..	..	..	1 10	2 10	..	..	..	..	..
	32	..	..	..	..	..	..	..	..	3 3	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..
	35	22	..	..	..	..	..	..	2 5	..	3 0	..	..	3 0	..	..	..	..	..	3 10	3 5	1 15	..	..	..	..	..	..

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a. Mounts, to special order, objectives as high as 1-10 inch, to work well on any binocular stand.

b. All of 1-5 or higher, either dry or immersion. Both ways with same lens, or with extra front, at extra price.

c. Higher powers to order.

d. Improved arrangement for thick covering-glass, 1*l*. 10*s*. extra.

e. All of 1-12 (No. 9) and higher, immersion and adjustable.

f. Changeable, by reversing, to 2¼ inches.

g. Changeable, by sliding tube, to 3 inches.

h. Divides to give 3 inches.

i. Has a front to give 1 inch of 25°.

j. Divides to give 2 inches.

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES—*continued.*

Nominal focus in inches.	Angular aperture in degrees.	Grunow.[a] Price in dollars. a.	Miller. Price in dollars. a.	Queen. Price in dollars. a.	Spencer. Price in dollars. a.	Tolles.[b] Price in dollars. a.	Wales.[c] Price in dollars. a.	Zentmayer. Price in dollars. a.	Baker. Price in pounds and shillings. b.	Beck. Price in pounds and shillings. c.	Browning. Price in pounds and shillings. d.	Collins. Price in pounds and shillings. e.	Crouch. Price in pounds and shillings. f.	Dancer. Price in pounds and shillings. g.	Horne & Thornthwaite. Price in pounds and shillings. h.	How. Price in pounds and shillings. i.	Murray and Heath. Price in pounds and shillings. j.	Pillischer. Price in pounds and shillings. k.	Powell and Lealand.[d] Price in pounds and shillings. l.	Ross. Price in pounds and shillings. m.	Steward. Price in pounds and shillings. n.	Swift. Price in pounds and shillings. o.	Wheeler. Price in pounds and shillings. p.	Hartnack.[e] Price in francs. q.	Gundlach. Price in thalers. r.	Merz. Price in thalers. s.	Wasserlein. Price in thalers. t.	Zeiss. Price in thalers. u.
	52	..	..	..	..	..	30f	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3 0	..	..	..	..	..	..
6–10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	9
1–2	..	..	..	..	..	..	..	..	..	..	..	3 10g	..	..	2 12½	..	2 10g	..	..	..	..	..	..	30	..	..	..	..
	30	..	..	..	..	23	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	8	..	..
	35	..	..	..	..	..	..	..	..	..	..	2 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	38	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3 10g	..	4	..	..	..
	40	..	..	..	20	23h	..	..	2 10	2 10	..	..	2 5	1 10	..	2 2	1 17½	..	..	..	..	2 5g	..	..	..	..	..	..
	55	..	18f	..	..	..	..	..	..	..	1 15	..	2 10g	..	..	..	..	..	..	..	..	..	3 0f	..	..	..	..	..
	56	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2 10	..	..	..	..	..	..	..
	60	..	..	..	..	35	..	..	3 0	..	..	..	..	..	..	..	2 7½	..	..	..	..	..	..	..	..	..	..	..
	65	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4 4	..	..	..	..	..	..	..	..	..	..	..	..
	70	..	..	..	..	..	..	..	..	..	..	3 10	..	4 0	..	..	3 3	..	5 0	..	..	..	..	..	..	..	..	..
	75	..	36	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4 4	..	..	..	..	..
	80	..	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	5 0	..	..	..	..	..	..	..	..	..	..
	85	..	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	90	..	..	..	..	..	..	..	..	..	..	..	3 15	..	..	..	4 10	..	..	5 5	4 0	3 15	..	..	..	..	..	..
4–10	25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4	..
	50	18	..	..	..	..	..	..	..	..	..	..	..		..	..	..	..	..	..	..	..	..	..	..	..	..	..
	55	..	..	..	..	..	..	..	..	5 5	..	4 0	4 0	..	..	..	..	4 0	..	..	..	..	..	..	..	..	..	..
	64	..	..	..	..	..	..	..	..	..	..	..	..	1 15f	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	70	..	..	..	..	..	..	..	3 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	75	25	..	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	4 0	..	..	..	..	..	..	..
	80	.	..	..	..	..	..	25	..	..	..	5 0	5 0	4 5	..	..	..	..	5 5	..	..	..	..	..	..	..	..	..
	85	..	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	90	..	..	..	45	45	40	..	..	7 7	..	..	..	..	..	..	..	..	..	..	..	4 0	..	..	..	..	..	..
	95	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	5 0	..	..	..	..	..	..	..	..	..	..
	110	35	..	..	..	45	40	..	..	..	..	..	..	..	..	..	..	..	..	6 6	5 10	..	..	..	..	..	..	..
	135	..	..	.	..	65	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	145	..	..	..	..	65i	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1–3	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	12
	30	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4	..

	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	8	..	..
	50	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4	..	..	..
1-4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3 3	..	..	..	..	..	..	..	..	35	..	..	..	..
	40	..	..	..	..	26[j]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6	..
	60	..	..	..	25[j]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	1 15	..	..	..	..	..	..
	70	..	18[f]	..	..	26[j]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	2[g]10[f]	..	..	..	..	..
	75	..	..	..	..	..	20[f]	..	3 5	5 5	..	..	2 2[j]	2 0[f]	..	2 2	2 0	..	..	..	2 10	2 2	..	..	..	..	..	..
	80	..	27[f]	..	35	..	26	..	..	..	..	..	..	..	..	..	2 7½	3 3	..	..	..	..	3 0[f]	..	6[f]	..	..	..
	85	..	..	..	..	..	..	..	..	..	2 0[f]	..	2 10[f]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	95	..	..	..	..	..	..	..	3 15	..	..	2 10	..	..	..	4 4	3 3	..	5 5	..	4 4	..	..	..	..	..	..	..
	100	..	40	..	..	40	..	..	..	..	..	..	3 0	..	..	..	4 10	5 0	..	5 5	..	3 0	4 4	..	..	..	..	..
	110	..	..	..	..	50	..	..	..	..	4 10	..	..	4 0	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	120	..	..	..	..	..	..	..	..	..	5 0	4 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	125	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	5 15	..	..	..	..	..	..	..
	130	..	..	..	..	50	..	..	..	..	..	..	..	..	..	..	..	..	7 7	..	..	4 0	..	..	..	..	..	..
	140	..	..	..	50	..	..	..	..	..	..	..	..	4 10	..	..	..	..	..	6 10	..	..	..	..	..	..	..	..
	145	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	8 8	..	..	..	..	..	..	..	..	..
	150	..	..	..	..	60	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	160	..	..	..	65	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	170	..	..	..	..	70	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	175	..	..	..	75[k]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	3 10	..	..	..	..	..	..	..	..	..	..	..	..	18
	60	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6[f]	..
	80	..	..	18[f]	35	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	85	..	..	..	..	..	..	..	..	5 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	90	20	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	100	..	..	..	..	40	..	..	..	6 6	..	..	3 0	..	..	..	..	..	6 6	..	..	3 0	..	..	..	..	..	..
	110	35	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	115	..	..	..	..	..	..	..	..	..	..	..	..	4 10	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	120	..	..	..	..	..	..	35	..	..	..	5 0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	130	..	..	..	..	50	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	5 0	..	..	..	..	..	..
	135	..	..	..	..	..	40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	140	40	..	..	50	..	..	..	..	..	..	..	..	5 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	150	..	..	..	..	60	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	5 10	..	..	..	..	..	..
	170	..	..	..	..	70	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	40	..	..	..	25
	90	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	7[f]	..
	100	..	..	..	..	45	..	..	..	..	..	..	..	..	..	4 0	3 17½	..	..	..	4 0	..	..	..	..	12	..	..

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a. Mounts, to special order, objectives as high as 1-10 inch, to work well on any binocular stand

b. All of 1-5 or higher, either dry or immersion. Both ways with same lens, or with extra front, at extra price.

c. Higher powers to order.

d. Improved arrangement for thick covering glass, 1*l.* 10*s.* extra.

e. All of 1-12 (No. 9) and higher, immersion and adjustable.

f. Non-adjustable.

g. Specially for binocular.

h. Specially arranged for opaque objects, 28 dollars.

i. Specially recommended as an achromatic condenser.

j. Adjustable by front lens.

k. Dry or immersion.

TABLE OF OBJECTIVES, THEIR MAKERS AND PRICES—*continued.*

Nominal focus in inches.	Angular aperture in degrees.	Grunow.[a] Price in dollars. a.	Miller. Price in dollars. a.	Queen. Price in dollars. a.	Spencer. Price in dollars. a.	Tolles.[b] Price in dollars. a.	Wales.[c] Price in dollars. a.	Zentmayer. Price in dollars. a.	Baker. Price in pounds and shillings. b.	Beck. Price in pounds and shillings. c.	Browning. Price in pounds and shillings. d.	Collins. Price in pounds and shillings. e.	Crouch. Price in pounds and shillings. f.	Dancer. Price in pounds and shillings. g.	Horn & Thornthwaite. Price in pounds and shillings. h.	How. Price in pounds and shillings. i.	Murray and Heath. Price in pounds and shillings. j.	Pillischer. Price in pounds and shillings. k.	Powell and Lealand.[d] Price in pounds and shillings. l.	Ross. Price in pounds and shillings. m.	Steward. Price in pounds and shillings. n.	Swift. Price in pounds and shillings. o.	Wheeler. Price in pounds and shillings. p.	Hartnack.[e] Price in francs. q.	Gundlach. Price in thalers. r.	Merz. Price in thalers. s.	Wasserlein. Price in thalers. t.	Zeiss. Price in thalers. u.
	130	..	54	..	..	55	..	..	..	..	..	..	..	..	..	..	..	6 0	..	..	..	..	6 6	..	..	..	..	..
	135	..	..	..	..	..	..	..	..	..	..	..	..	..	..	6 0	..	..	..	..	6 0	..	..	..	..	..	..	..
	140	..	..	..	..	65	..	..	..	..	..	..	..	..	..	..	6 0	..	..	7 7	..	..	..	..	..	..	..	..
1–7	100	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	7[f]	..
1–8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	4 4	..	..	..	..	..	..	..	..	..	..	..	..	..
	70	..	..	..	..	35[g]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	85	..	..	..	..	..	..	..	..	5 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	95	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	5 5	..	..	..	..	..	..	..
	100	..	..	..	..	45	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	115	..	..	..	..	..	..	..	5 5	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	9[f]	..
	120	..	..	..	..	..	..	..	..	8 8	..	..	..	6 10	..	5 0	4 12½	..	..	..	..	..	..	..	..	..	..	..
	125	..	..	..	..	..	..	..	6 6	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	130	..	..	..	..	..	..	..	..	..	..	4 4	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	135	..	..	..	..	..	..	..	..	..	7 0	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	140	45	72	..	..	65	..	..	..	..	..	6 10	4 10	7 7	..	..	7 10	7 10	8 8[h]	8 8[i]	..	4 10	8 8	..	..	..	..	..
	145	..	..	..	..	..	45	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	150	45	..	..	60[f]	..	..	..	..	..	..	..	..	..	..	7 7	..	..	..	..	7 7	..	..	..	10[f]	..	..	..
	160	..	..	..	..	75	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	175	..	..	..	80[f]	80	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1–9	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	50	..	..	..	..
	120	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	16	..	..
1–10	..	..	..	..	..	..	..	..	..	6 0[k]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	100	..	..	..	..	45	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	125	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	12[f]	..
	130	30[k]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	160	50[k]	..	..	..	80	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	170	..	..	..	..	..	60	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	175	..	..	..	..	85	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1–11	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	75	..	..	..	..
1–12	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	5l 0[k]	..	150	..	..	..	..
	120	..	..	..	..	60	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	16	..	..

	140	..	..	..	..	80	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	145	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	12 12[h]	..	..	..	..	..	..	..	..	..
	160	..	..	..	..	100	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	170	..	..	..	..	115	..	..	..	..	..	..	..	..	..	..	..	..	..	12 12[m]	12 12	7 10	..	..	20	..	..	..
	175	..	..	..	100[j]	115	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-15	100	..	..	..	..	70	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	140	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	24[k]	..	..
	160	75[b]	..	..	..	120	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	170	..	..	..	..	..	75	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	175	..	..	..	..	125	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-16	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	200	..	..	..	..
	140	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	15	..
	175	..	..	..	120[j]	..	..	..	..	..	..	..	..	..	..	..	..	..	16 16[h]	..	..	..	..	..	20[k]	..	..	..
1-18	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	250	..	..	..	..
	150	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	32[b]	..	..
1-20	..	..	..	..	..	..	..	..	..	..	..	..	..	..	15 15	..	..	..	..	..	..	..	..	..	..	..	..	..
	100	..	..	..	..	100	..	..	..	[illegible] 8	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	140	..	..	..	..	..	..	..	..	1[illegible] 15	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	150	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	15 10	..	..	..	..	..	..	..	..	..	..
	175	..	..	..	150[j]	180	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-21	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	300	..	..	..	..
	160	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	40[k]	..	..
1-24	170	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	56[k]	..
	175	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	30[k]	..	..	..
1-25	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	350	..	..	..	..
	155	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	25[k]	..
	160	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	21 0	..	..	..	..	..	..	..	..	..
1-28	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	400	..	..	..	..
1-30	..	..	..	..	..	*n*	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
	160	..	..	..	..	..	120	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-32	175	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	45[k]	..	..	..
1-33	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	450	..	..	..	..
1-40	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	500	..	..	..	..
1-45	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	500	..	..	..	..
1-50	..	..	..	..	..	*n*	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	500	..	..	..	..
	150	..	..	..	..	225	..	..	..	..	..	..	..	..	..	..	..	..	31 10	..	..	..	..	..	..	..	..	..
	175	..	..	..	200[j]	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..
1-75	..	..	..	..	..	*n*	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..	..

a. Mounts, to special order, objectives as high as 1-10 inch, to work well on any binocular stand.
b. All of 1-5 or higher, either dry or immersion. Both ways with same lens, or with extra front, at extra price.
c. Higher powers to order.
d. Improved arrangement for thick covering-glass, 1*l*. 10*s*. extra.
e. All of 1-12 (No. 9) and higher, immersion and adjustable.
f. Non-adjustable.
g. Adjustable by front lens.
h. Immersion arrangement, 2*l*. 2*s*. extra.
i. Extra front for immersion, 2*l*. extra.
j. Dry or immersion.
k. Immersion.
l. Extra front, for dry 1-8, 2*l*. extra.
m. Extra front for immersion, 2*l*. 10*s*. extra.
n. By special contract.

SYNOPSIS OF STUDENTS' MICROSCOPES.

MODEL.

Must have joint to incline at various angles from horizontal to perpendicular.

Curved Bar.—"Jackson" (but not prolonged beneath stage); steadiest form for equal weights.

Transverse Bar.—Most convenient in some respects.

WEIGHT.

Four to ten pounds.—A question of convenience *v.* firmness.

HEIGHT. (12 to 16 inches.)

Tall.—Much room below stage. Standard length of tube gives usual powers. Looks well, and is most convenient when inclined.

Short.—Easiest for vertical use. Short tube works lenses at too low power. Should be lengthened by draw-tube when inclined.

DIAMETER OF TUBE. (1 to 1 3-8 in.)

Small.—Looks best and is steadiest on small instruments.

Large.—Gives wider field with low-power oculars.

MATERIAL.

All Brass.—Looks best.

Iron Base.—Saves expense. Wears well unless broken by falling.

COARSE ADJUSTMENT.

Rack and Pinion.—Easiest; preferable, but costly.

Friction Pinion.—Said to be very delicate.

Chain Movement.—Fine motion. Easily repaired if necessary.

Sliding Tube, by hand.—Saves expense. Most delicate, but awkward. Wants expert hands, and often two of them.

FINE ADJUSTMENT. Essential.

Screw and lever moving nose-piece.—Best. Costly.

Screw and lever moving compound body.—Good for moderate powers.

Screw moving compound body.—Less delicate.

Screw against shoulder, moving nose-piece.—Less delicate.

Screw on nose-piece.—Little used.

Screw moving stage.—Cheapest in use in this country. Bad in theory and history; but quite satisfactory in use, as made at present.

Bar moving pinion of rack.—Not in this country. Good for low powers. May be added to instruments having no other fine adjustment.

STAGE.

Mechanical.— Not required.

Lever.— Plausible, but not successful.

Magnetic.—Plausible, but not successful. (Insecure.)

Hand movement. (In two rectangular directions.)—Best substitute for mechanical, for high powers, and with Maltwood finder.

Glass, concentric.—Best for binocular.

Glass, sliding by hand.—Cannot be too highly commended for ordinary medical use.

Brass.—Sliding by hand (cloth-lined, or otherwise). A good and cheap substitute.

Plain.—Saves expense. Should have removable spring clips. Could have glass or brass sliding-stage added.

MIRROR.

Must have lateral, vertical, and two rotary movements.

Lateral movement by cranked arm: not common in students' microscopes. Lateral movement by mirror swinging partly around the bar which carries it. Lateral movement by hinge joint at top of bar which carries mirror: obliquity entirely independent of distance of mirror from object.

Concave.—Indispensable. Should be nearly two inches wide.

Plain.—Very desirable, and costs little. May be smaller.

DIAPHRAGM.

Fitting should be removable, for oblique light.

Disk with apertures.—Essential. Should be closed between diaphragm and stage to prevent accidental oblique light.

"*Graduating*" or "*Iris.*"—Incomparably best, but unnecessarily expensive.

OCULARS (eye-pieces).

Negative (Huyghens') always meant, unless otherwise stated. Quality (and performance) varies greatly, though seldom suspected.

Two-inch.—(The A or No. 1 of most makers.) X* 5. Best for general use. Should always be negative.

One-inch.—(Often named B or C, or No. 2 or 3.) X 10. Use for micrometry, occasionally for observations, and generally as achromatic condenser. If willing to add $5 or $10, substitute Kellner's orthoscopic, by European or American makers (superb field), or Tolles' orthoscopic, by Boston Optical Works (field narrower, but peculiarly clear).

OBJECTIVES. (If extravagant anywhere, put it here.)

All above 50° angular aperture, should have adjustment for cover glass.

All above about 75° must have the same.

Screw collar.—Best.

Screw movement of outer tube.—Good for cheap lenses of small angles.

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* Magnifies, diameters.

Sliding movement of outer tube, with oblique slot.—Good for cheap lenses of small angles.

One-inch.—X 10 (with lowest ocular, X about 50). Should be about 25° angular aperture.

One-fourth or one-fifth inch.—X 40 or 50 (with low ocular, X about 200 or 250). May be 75° to 85°.

One-sixth to one-eighth inch.—X 60 to 80. (With low ocular, X about 300 to 400.) Should be 120° to 140°. Best added after a few years' experience with the others.

Magnifying Powers.

Quality more than quantity.

Theoretical.—One inch X 10. Oculars and adjectives as above, X 50, 100, 200, 400, 800.

Practical.—Should approximate to this.

Accessories.

Essential, and should be furnished free.

A few glass slides and covers; stage-plates with ledge; pipettes; pliers; and needles mounted in handles.

Desirable, for medical and general use.

Compressorium, or *animalcule cage.*

Curved scissors (sideways).

Camera lucida, for drawing and micrometry.[*]

Stage micrometer.

Graduated draw-tube.

Cylindrical fitting, below stage, to use ocular for achromatic condenser.

Reagents. (In twelve one or two ounce vials.)

Turntable, and mounting materials.

Desirable, more for general than medical use.

Condensing lens, on separate stand, for opaque objects, and for parallel light, &c.; or *condensing lens* to limb or stage of instrument, for opaque objects; or *mirror on curved arm*, to swing above stage, for opaque objects; or *mirror removable* to stage or separate stand for opaque objects.

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[*] For occasional measurements the camera is entirely satisfactory. Where large numbers of measurements are to be made, this method is not sufficiently rapid and convenient. Dr. White's micrometer, a semicircle of thin glass, graduated at the straight edge, and lying upon the diaphragm in focus of the eye lens, is most used by the writer for this purpose. With low powers the object is easily brought up obliquely to the required position; for high powers the Jackson adjusting screw must be used, unless the stand have a mechanical stage. The simplest ocular micrometer, and best for general students' use, is a circular disk cut, to fit, from the centre of a rather thin stage-micrometer graduated to hundredths or to two-hundredths of an inch, and lying, only when used, upon the diaphragm in the ocular like Dr. White's form. This rather thick circle is more durable, more easily obtained and handled, and capable of giving unexpectedly good results. It should cost $2 or $3.

Spotted lens, or glass paraboloid, for translucent objects.

Stage forceps.

Maltwood finder.

PRICE.

Including walnut or mahogany case, $50 to $100. Varies with quantity and quality of work and reputation of maker.

Less, at present, unsatisfactory.

More, unnecessary.

About double, by substituting binocular stand.

TABLE OF AMERICAN

Name and Address of Maker.	Name of Instrument.	Model.	Height (in inches) with high powers.	Weight (in lbs.).	Inside diameter of tube (in inches).	Material (Brass except as stated).	Coarse Adjustment.
Boston Optical Works.* Charles Stodder, agent, 66, Milk Street, Boston.	Tolles' Students' Microscope.	Jackson.†	$15\frac{1}{2}$	6	·96	Iron base and arm.	Sliding tube with steel spring.
Boston Optical Works.* Charles Stodder, agent, 66, Milk Street, Boston.	Tolles' Students' Microscope.	Jackson.†	$15\frac{1}{2}$	6	·96	Iron base and arm; $10 extra for all brass.	Rack and pinion.
J. Grunow, 410, Fourth Avenue, N. Y.	Students' Microscope (horse-shoe base).	Transverse bar.	12	5	$\frac{31}{32}$	Brass.	Sliding tube.
J. Grunow, 410, Fourth Avenue, N. Y.	Students' Microscope (horse-shoe base).	Transverse bar.	12	5	$\frac{31}{32}$	Brass.	Sliding tube.
T. H. McAllister, 49, Nassau Street, N. Y.	Students' Microscope.	Transverse bar.	12	$4\frac{1}{2}$	1	Iron base.	Chain movement.
T. H. McAllister, 49, Nassau Street, N. Y.	Professional Microscope.	Transverse bar.	15	7	$1\frac{5}{16}$	Iron base.	Chain movement.
Wm. Y. McAllister, 728, Chestnut Street, Philad.	(No. 5374).	Transverse bar.	13	$4\frac{3}{8}$	$1\frac{1}{16}$	Iron base.	Sliding tube.
Wm. Y. McAllister, 728, Chestnut Street, Philad.	(No. 5374).	Transverse bar.	13	$4\frac{3}{8}$	$1\frac{1}{16}$	Iron base.	Sliding tube.
Miller Bros., 1223, Broadway, N. Y.	Students' Microscope.	Jackson.	16	5	$1\frac{1}{4}$	Iron base and arm.	Sliding tube, in cloth.
Miller Bros., 1223, Broadway, N. Y.	Educational Microscope.	Transverse bar.	15	4	$1\frac{3}{16}$	Brass.	Rack and pinion.
B. Pike's Son, 518, Broadway, N. Y.	Students' Microscope.	Jackson.	15	5	$1\frac{3}{8}$	Iron base and arm.	Rack and pinion.
B. Pike's Son, 518, Broadway, N. Y.	Physicians' Microscope.	Jackson.	15	5	$1\frac{3}{8}$	Iron base.	Chain movement.

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* R. B. Tolles, superintendent.

† Arm expands to fill whole space between uprights, and gives, with conical steel pin, a very simple trunnion joint.

STUDENTS' MICROSCOPES.

Fine Adjustment.	Stage.	Mirror.	Oculars.	Objectives.	Range of Magnifying Powers.	Accessory Apparatus.	Price with case (in dollars).	Price of cheapest binocular stand only (in dollars).
Screw to stage.	Plain, with spring clips.	Concave, 1⅞ in., with side movement.	1⅓ in.	1 in. and ¼ in.	**75** to **300**	Mirror to separate stand for opaque illumination.	**50**	**80,** *extra* for binocular eye-piece.
Screw with lever to nose-piece.	Plain, with fitting for accessories.	Plane and concave, 1⅞ in., with side movement.	1⅓ in.	1 in. and ¼ in.	**75** to **450**	Mirror to separate stand for opaque ill., draw-tube, and camera lucida.	**100**	**80,** *extra* for binocular eye-piece.
Screw to compound body.	Plain, with spring clips.‡	Plane and concave, 1⅝ in., with hinge movement.	Two.	⅔ in. **25°**, and ⅕ in. **90°**.	**70** to **500**	None.	**85**	None.§
Screw to compound body.	Plain, with spring clips.‡	Plane and concave, 1⅝ in., with hinge movement.	Two.	⅔ in. **25°**, and ⅕ in. **90°**.	**70** to **500**	Camera lucida, stage micrometer, & compressorium.	**100**	None.§
Screw to stage.	Plain, with spring clips.	Plane and concave, with hinge movement.	B.	1 in. and ¼ in. (French).	**50** to **400**	Draw-tube and stage forceps.	**50**	None, at present.
Screw with lever to nose-piece.	Glass, sliding by hand.	Plane and concave, with hinge movement.	A. & B.	1 in., ½ in., and ¼ in.	**50** to **600**	Draw-tube and stage forceps.	**100**	None, at present.
Screw with lever to compound body.	Glass, sliding by hand.	Concave, with hinge movement.	Two.	One set of French, dividing.	**50** to **350**	Condensing lens on stand.	**50**	None.
Screw with lever to compound body.	Glass, sliding by hand.	Concave, with hinge movement.	Two.	8/10 in. **32°**, and ⅕ in. **120°** (Zentmayer).	**50** to **430**	Condensing lens on stand, camera lucida, stage micrometer, and animalcule cage.	**100**	None.
Screw to stage.	Plain, with spring clips.	Concave, with hinge movement.	A. & B.	1 in. **16°**, and ¼ in. **75°**.	**37** to **310**	Mirror to stage for opaque ill.	**60**	**100**
Screw with lever to nose-piece.	Plain, with spring clips.	Concave, 1⅝ in., with side movement.	A. & B.	1 in. **16°**, and ¼ in. **75°**.	**37** to **310**	Stage plates and condensing lens on stand.	**80**	**100**
Screw with lever to stage.	Glass, sliding by hand.	Plane and concave.	A. & B.	1 in. and ⅕ in. (Gundlach).	**50** to **600**	Condensing lens on stand.	**75**	**125**
Screw with lever to compound body.	Glass, sliding by hand.	Plane and concave.	A. & B.	1 in. and ⅕ in. (Gundlach).	**50** to **960**	Condensing lens on stand, and draw-tube.	**100**	**125**

‡ Diaphragm, sunk into upper surface of stage, so as to be close to the object slide.
§ Makes optical parts, to work with Zentmayers's binocular hospital-stand.

TABLE OF AMERICAN

Name and Address of Maker.	Name of Instrument.	Model.	Height (in inches) with high powers.	Weight (in lbs.).	Inside diameter of tube (in inches).	Material (Brass except as stated).	Coarse Adjustment.
James W. Queen and Co., 924, Chestnut Street, Phil., and 535, Broadway, N. Y.	Students'† Microscope (No. 1665).	Jackson.	14	5	1⅜ (scant).	Iron base and arm.	Sliding tube.
James W. Queen and Co., 924, Chestnut Street, Phil., and 535, Broadway, N. Y.	Students'† Microscope (No. 1665).	Jackson.	14	5	1⅜ (scant).	Iron base and arm.	Sliding tube.
Chas. A. Spencer and Sons, Canastota, N. Y.	Students' Microscope.	Jackson.	15	6	1·18	Iron base and arm.	Sliding tube, in velvet.
Chas. A. Spencer and Sons, Canastota, N. Y.	Students' Microscope.	Jackson.	15	6	1·18	Iron base and arm.	Rack and pinion.
Chas. A. Spencer and Sons, Canastota, N. Y.	Standard Microscope.	Jackson.	17	11	1·18	Iron base and arm.	Rack and pinion.
Wm. Wales,* Fort Lee, N. J.							
J. Zentmayer, 147, South 4th Street, Philadelphia.	Students' Microscope (round base).	Jackson.	15	5½	1⅜	Brass.	Sliding tube.
J. Zentmayer, 147, South 4th Street, Philadelphia.	Students' Microscope (round base).	Jackson.	15	5½	1⅜	Brass.	Sliding tube.

* Accessories of Beck's "popular" stand fits this.
† Makes optical parts only, to stands by any maker.

STUDENTS' MICROSCOPES—*continued.*

Fine Adjustment.	Stage.	Mirror.	Oculars.	Objectives.	Range of Magnifying Powers.	Accessory Apparatus.	Price with case (in dollars).	Price of cheapest binocular stand only (in dollars).
Screw with lever to nose-piece.	Glass, sliding by hand, with fitting for accessories.	Plane and concave, with hinge movement.	A.	⅛ in. (dividing).	**100** to **300**	Stage micrometer and condensing lens on stand.	**60**	None.
Screw with lever to nose-piece.	Glass, sliding by hand, with fitting for accessories.	Plane and concave, with hinge movement.	A. & B.	**1** in. **18°**, and ⅛ in. **80°**.	**50** to **500**	Stage micrometer, condensing lens on stand, camera lucida, and compressorium.	**100**	None.
Screw to stage.	Plain, with spring clips.	Plane and concave, 1¾ in., with hinge movement.	B.	**1** in. **20°**, and ¼ in. **60°** (adj.).	**85** to **325**	Mirror on curved arm to swing above stage for opaque ill.	**60**	None.
Screw with lever to nose-piece.	Plain, with spring clips.	Plane and concave, 1¾ in., with hinge movement.	B.	**1** in. **20°**, and ¼ in. **60°** (adj.).	**85** to **325**	Mirror on curved arm as above, camera lucida and animalcule cage.	**100**	None.
Screw with lever to nose-piece.	Plain, with spring clips.	Plane and concave, 2 in., with hinge movement.	B.	¼ in. **60°** (adj.).	**325**	Mirror on curved arm as above.	**100**	None.
Screw to stage.	Plain, with spring clips, and tube for accessories.	Plane and concave, 1⅞ in., with hinge movement.	A. & B.	8/10 in. **22°**, and ⅛ in. **85°**.	**50** to **430**	Condensing lens to limb.	**75**	**135**
Screw to stage.	Plain, with spring clips, and tube for accessories.	Plane and concave, 1⅞ in., with hinge movement.	A. & B.	8/10 in. **22°**, and ⅛ in. **85°**.	**50** to **430**	Condensing lens to limb, camera lucida, stage micrometer, & compressorium.	**90**	**135**

PROGRESS OF MICROSCOPICAL SCIENCE.

American Desmids.—'Grevillea,' which is the title of the admirable little monthly record of cryptogamic botany issued by Mr. M. C. Cooke, M.A., the well-known fungologist, states that the seven new species of Desmids described by Dr. Wood in the 'Proceedings of the Academy of Natural Sciences,' at Philadelphia, are named by him—*Euastrum multilobatum*, *Euastrum ornatum*, *Arthrodesmus quadridens*, *Staurastrum minutum*, *Staurastrum Lewisii*, *Cosmarium suborbiculare*, and *Pleurotœnium breve*. Descriptions in Latin, with measurements, will be found in 'Hedwigia' for 1872, No. 1, p. 3.

The Microscopic Action of Quinine and Arsenic.—In a paper read before the Biological Section of the Academy of Natural Science, America, Dr. Joseph J. Richardson made some important remarks on the action of certain medicines in completely removing microscopic growths in the skin. He said in conclusion that it is not difficult to found a theory for explaining the probable mode of operation of quinine and arsenic—so long a problem in therapeutics—when acting as tonics upon the human system in many cases of disease: namely, that in part, at least, they serve their important purpose by rendering the blood less fit for the development of these lower organisms, and in this way *economizing* the supply of nutritive material in the circulating fluid. Many well-known peculiarities in the action of these two principal tonic medicines tend to confirm such a belief—as, for example, the facts: 1st, that quinine and arsenic, two substances which, although unlike in almost every other respect, resemble each other, as shown by the experiments of Dr. Binz of Bonn, and Dr. Dougall, of Glasgow, in being powerfully inimical to vegetable life, are in many cases our most reliable tonics. 2nd, that, as a general rule, three or four days must elapse before a decided invigorating effect is produced upon the system by these remedies—a period which, we may conclude, is requisite for sufficiently impregnating the blood (without disturbing the digestive organs) to render it an unfavourable medium for the growth of Bacteria. 3rd, that after a continuance of some weeks these roborants generally lose their invigorating power upon any particular patient, but seem to have regained it if recommenced after their administration has been for a short time interrupted—*i. e.* subsequent to the development of a new crop of Bacteria, which they again destroy. And 4th, that arsenious acid has so remarkable an effect upon the arsenic-eaters of Styria, producing such fat, vigorous, and pure-complexioned individuals when steadily continued, and giving rise to such intense suffering if omitted for a short time; results which can hardly be owing to the arsenical preparation supplying any necessary constituent of the human body, and which seem much more probably to be due to some action in preventing a waste of nutriment, and consequently of vital power.

A Minute Nostoc with Spores.—In Mr. Cooke's 'Grevillea,' July, appears an interesting paper by Mr. W. Archer, of Dublin, on (probably) *Nostoc paludosum.* This species is a very minute one, though the dimensions of the subglobose or elliptic fronds vary much. It is rather common in moor and certain bog pools. On account of its small size, therefore, readily capable of compression, and its pellucid character, the elegant arrangement of its tortuously-twisted rather large moniliform filaments, is often nicely seen, and this causes it to be a very pretty and favourable illustrative example of its type for examination in its entirety under the higher powers of the microscope. Its minute size calls to mind *Nostoc minimum* (Currey), but in it the cells are described as quadrate with a sinus at each side, lending a crenate outline to the filaments, and the heterocysts are large, whilst here the cells are orbicular, or for a time slightly flattened at the junctions, and the heterocysts are but slightly wider, though longer than the ordinary cells. This plant is probably identical with *Nostoc paludosum* (Kütz.), though as regards anything to be deduced from the heterocysts Kützing is silent. But the interesting point connected with it is a single example of it having presented indubitable "spores," and precisely similar nature to those in Sphærozyga, &c., but with the peculiarity of these being always placed singly between two heterocysts. The pairs of heterocysts with the intervening spore occurred at just about the same intervals as in ordinary examples occur the isolated heterocysts; the spores large, broadly elliptic, about one-third longer than broad; their diameter more than twice the diameter of the heterocysts, about thrice the diameter of the ordinary cells; the "bright points" of the heterocysts not very conspicuous.

Development of the Skull of Salmo salar.—Our President, Mr. W. K. Parker, F.R.S., has just given the Royal Society a most elaborate and wonderful account of the development of the skull of this fish. We cannot be expected to do more than give an outline of the author's mass of facts so patiently and accurately worked out. His last subject, the frog, being fairly out of hand, Mr. Parker says that he set himself last summer to this "newer and more easy task—more easy by far, for the translucency of the young salmon contrasts most favourably with the obscurity of the embryo frog." He found that the two types at the time of hatching did not start fairly, but that the salmon had hastened to finish its *fourth stage* before emerging from the egg; this, however, is partly in consequence of the difference of the envelope in which the embryos are contained, for in the salmon this is a leathery "chorion," and in the frog a mere gelatinous bleb. Moreover, it soon became apparent that these two "Ichthyopsidans" are in nowise near akin to each other. In the very first stage, where there is an essential agreement, in one important particular they greatly disagree; for the embryo of the salmon has two arches in front of its mouth, while the tadpole has but one; there is also an additional gill-arch in the osseous fish. In the earliest stage of the salmon worked out by him he found a much more distinct condition of the parts than in frogs at the same stage; the differentiation of the latter is obscure as compared with the fish, and this not merely because of the quantity of *pigmentum nigrum* in the tissues of the former. Then, in addition to other causes of obscuration, the mouth of the tadpole is strangely modified in harmony with its "suctorial" character and affinities (showing a remarkable affinity to the mouth of a lamprey), so that a whole system of cartilages has to be eliminated from the lips before the mouth (proper) can be understood. The labial system is slightly and slowly developed in the salmon, and its mouth is thus much more in harmony with that of the embryo reptile or bird than with that of the tadpole. After the simple stage is passed, the development of the facial arches is very different in the two types, as different, indeed, as in any two possible examples that could be given in the whole vertebrate group. The facial arches behind the mouth now undergo segmentation; first the hyoid, and then the mandibular. The hyoid is cloven from top to bottom, and also has a single distal piece separated off. "At this stage we get an explanation of what is seen in certain rays, where the hyoid suspensorium is permanently double; and also ascertain that this second postoral arch, which retains the anterior piece in relation to the skull as the great 'hyomandibular' pier, does not need the *saw* of the transcendentalist to put it into proper relation to its surroundings. Nature's invisible *wedge* has done what was needed, and the supposed *double rib* turns out to be *half* a visceral arch. On the whole, this second stage is extremely 'Plagiostomous,' for the details of which I must refer to the main paper." While in the egg the head of the embryo is flattened, and so twisted that one of the eyes (it may be the left or the right) looks upwards towards the "chorion," the other having a *visceral* direction. The facial bars, at first having all a simple sigmoid form, rapidly change towards the time of hatching, and, when the head gets free, the cerebral vesicles speedily swell, taking on the form so familiar to the embryologist; and the head now gains the "mesocephalic flexure." "After this an approach is made to the Teleostean type of structure; but this is not done at a stride. The intermediate condition is thoroughly 'Ganoid,' and, happily, comes in to explain the related structures of the *older* and *newer* 'Orders.' I am not aware that any stage of the heart or of the intestines shows either the many valves of the 'aortic bulb' or the intestinal spiral valve; this must be seen to; yet if these never show themselves in the 'fry' of the osseous fish, their absence does not affect the general skeletal morphology. The salmon amongst fishes, like the fowl amongst birds, never attains to the greatest degree of special-class-modification; it remains *subtypical*, with a dentigerous maxillary, a ductus pneumaticus, a very *chondrosteus* state of the skull, and a very heterocercal tail. Yet, from an ichthyological point of view, this fish is an immense height above the Sharks and Rays, and is far in advance as a fish of the whole group of 'Ganoids.' The results of the *gradational* study of the fish-forms by the zoologist, and of their *secular* study by the palæontologist, are both in harmony with morphological facts. Although the light obtained is but as the first streak of dawn, yet it is a pleasant light, and quite sufficient to show each kind of worker where and how to renew his own special toil. I cannot close this brief abstract without remarking that my researches in these, the highest types of animals, seem to me to be in perfect accordance with the results obtained by long study of the very lowest, the Rhizopods—namely, that they both yield increasing evidence in favour of the doctrine of Evolution. Researches of this kind show what the life processes can accomplish in the history of one individual animal, and also that the morphological steps and stages are not arbitrary, but take place in a manner in accordance with all that has of late been revealed to us of the gradation of types in the ages that are past."

Influence of Quinine on the White Blood Cells.—An article in a recent number of the 'Practitioner' deals with this subject. The paper is by Dr. Geltowsky, which derives additional interest from being the result of work done at the Brown Institution. The author confirms the statements of previous observers that the addition of a minute quantity of sulphate or hydrochlorate of quinine to drawn blood arrests the movements of the colourless corpuscles, either instantaneously or after a time, according to the quantity used; and he establishes that the action is due to the salt of quinine itself, not to its solvents; and that it is not shared by methyl sulphate of strychnia, urea, or chloride of sodium. Proceeding further from these data, he estimated the quantity of quinine that would be required to affect the whole mass of the blood in a given animal, and injected this quantity into a vein. In every instance either narcotism or death was produced; and the corpuscles, notwithstanding, remained wholly unaffected.

NOTES AND MEMORANDA.

Nomenclature of Objectives.—This controversy goes on still, and we are in hopes that it will prove of value to microscopists by rendering the terminology of the different countries at length identical. In the 'American Naturalist' for June, a writer signing himself C. S., who we doubt not is Mr. Charles Stodder, says of Dr. Ward's paper in the same journal that its author well says of the prevailing practice, "To call two lenses of identical magnifying power respectively one-fourth and one-sixth inch lenses, is just as indefensible as to call two houses of equal height, forty and fifty feet high respectively." An apt illustration of this is afforded by Dr. J. J. Woodward's paper in the April number of the same journal, where he cites an instrument invoiced $\frac{1}{30}$, which by actual measurement at the "open point" was only a $\frac{1}{18}$? But other objectives by the same maker called $\frac{4}{10}$, are known to be nearly as short focus as $\frac{1}{4}$ or less than $\frac{1}{3}$, showing that no system is used in the nomenclature. Other instruments from other makers have given similar results. With such discrepancies, and confusion, microscopists have nothing to depend on in ordering or comparing their instruments. The points from which the measurements are to be made is the question which is most obscure. Undoubtedly the best, and the true theoretic plan is to measure from the optical centre of the objective to the optical centre of the ocular, or the conjugate focus. This is the method of one maker of objectives. Practically it is next to impossible to apply the rule with mathematical exactness. Mr. Cross says, "because the compound objective has no optical centre;" but this is an error; several lenses combined must have an optical centre as truly as a single lens; but the difficulty is to find its place among the lenses first, and then to find where that place is on the outside of the tube. But for high-power objectives it is so near the point between the front and middle combinations, that if that point is taken, there can be hardly an appreciable difference from the truth in the result. The optical centre of the ocular is the diaphragm, if that is in the place where it should be. But in the modern objective the optical centre is a movable point by the collar for cover adjustment. The nearer the lenses are brought together the more the combination magnifies. Shall the objective be named at least power, or some other? He says the least, because it is the fixed point, always to be found; because it represents the least the instrument can do, and the owner knows that the power increases from that point; and because it seems to have been adopted in theory by most of the best makers, as but few of their *medium* powers have ever been found "over-named." If they did not proceed on that principle, their objectives were still farther from what they were named than they have been found to be by that rule. It would certainly be desirable that makers should give the minimum as well as the maximum of focus for each objective. The practice of most makers seems to have been to make certain lenses by rule of thumb, *to be* called *e.g.* a fourth, put them together, give them the name, and sell them without any test of what their real focus should prove to be. Angular aperture varies also with the change of cover adjustment, and the practice is to give that at its maximum only; Tolles, now, however, gives and marks sometimes both maximum and minimum. Dr. Ward suggests that it should be given at the same point as the name. It is usually at the minimum there; but it may be anywhere else; Tolles can make it the maximum at open point. The question has been raised in regard to objectives with two front lenses, interchangeable, of different powers, as to what they should be called. Obviously they are practically two instruments, and should have the names proper to each front. This may be illustrated thus: Tolles will make an objective dry $\frac{1}{10}$, and to that put another front $\frac{1}{20}$ immersion, would it not be preposterous to call the performance of the immersion front that of a $\frac{1}{10}$th? His practice is to give two names; and as the originator of the plan of two fronts he may have the right to fix the rule of nomenclature.

The Biological Secretaryship of the Royal Society.—In this we of course are interested as well as the macro-biologists. The loss of Dr. Sharpey will be a very great one; for with his great and extended knowledge he possessed a most kindly and painstaking manner, which we ourselves have frequently experienced. We trust that his post may be filled by an equally worthy person, and assuredly we know of none so highly qualified for the office as Professor Huxley, whom we hope to see soon in Dr. Sharpey's place.

CORRESPONDENCE.

Brighton Meeting of British Association.*

To the Editor of the 'Monthly Microscopical Journal.'

Royal Pavilion, Brighton, *July* 19, 1872.

Sir,—I have the sanction of the General Manager of the London, Brighton, and South-Coast Railway, to state that he will be willing to extend to the members of your Society the same railway facilities as will then be given to the members of the British Association during their visit to Brighton in August next.

In a day or two you shall hear further from me.

I have the honour to be, Sir,
Very faithfully yours,
James Beck,
Secretary to Executive Committee.

Walter W. Reeves, Esq.,
Assist.-Secretary, Royal Microscopical Society.

The Measurement of Covering Glass.†

To the Editor of the 'Monthly Microscopical Journal.'

Rochefort-sur-Mer, *June* 7, 1872.

Sir,—In an article inserted in the last May number of the 'Monthly Microscopical Journal,' and entitled "Optical Curiosities of Literature," the Rev. S. Leslie Brakey points out an error in a note of mine published in August, 1871, in your estimable Journal. That note relates to the thickness of covering glass and of objects, and to the manner of measuring them. I remarked that error myself very soon afterwards; but as the measurement of thin glass and of objects was not the exclusive subject of the note, I did not think it necessary to make a correction of it, but left to each micrographist the care of detecting the error now pointed out by the Rev. L. Brakey, and which I admit in all humility. I admit in fact that the thickness of objects cannot be ascertained by a change of focus, owing to the law of the *transport des images.* The same is true respecting the thickness of thin glass, which cannot be measured by transparency, because the lower surface *appears* to rise about $\frac{1}{3}$th (?). This phenomenon occurs with every object placed under a thin plate, according to the indices of refraction of the thin glass, of the liquid, and of the object itself.

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* Mr. Reeves has requested us to publish this letter, which we do with great pleasure. Mr. Beck deserves the thanks of Fellows of the Society.

† Some points in this letter are not particularly clear, but the translator is by no means to blame.—Ed. 'M. M. J.'

In any case I thank the Rev. L. Brakey for the courteous way in which he has pointed out the error committed.

As regards the application of my apparatus to the determination of vision, the chief object of my note, I think its exactness cannot be disputed, and up to the present the arrangement has appeared to me to constitute a perfect optometer.

Be pleased to receive, Sir, the assurance of my most distinguished consideration.

MOUCHET.

PROCEEDINGS OF SOCIETIES.*

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

March 14th.—Ordinary Meeting. Mr. Hollis, President, in the chair. Messrs. B. Lomax, Mills, Pinker, and H. Verrall were elected ordinary members.

Mr. Wonfor announced the receipt for the Society's library of 'Catalogue of Cretaceous Fossils,' in the Brighton Museum; 'The Willett Collection,' from Mr. H. Willett; 'Geological Notices of North Hampshire,' by Dr. Stevens, from the author; 'Proceedings of the Eastbourne Natural History Society,' from the secretary; and for the Society's herbarium, from Mr. G. Davies, of a lichen new to Sussex, *Bacidia muscorum*, obtained by that gentleman a few days before at Patcham; and a moss new to Sussex, *Fissidens decipiens*, from the moist part of the Downs above Slindon. Though common in mountainous limestone districts, it had hitherto escaped detection in Sussex, being confused with *F. adiantoides*, the large lax round cells of whose leaves differed from the pale-margined, densely-areolated leaves of *F. decipiens*. This moss had been found by Mr. Davies.

Votes of thanks were given to the donors.

Mr. F. E. Sawyer then read a very interesting paper "On Rain, with special reference to the rainfall of Sussex, and how it is influenced by the South Downs."

A vote of thanks was given to Mr. Sawyer for his paper.

Mr. Sewell exhibited a series of fossils from the Whincopp collection, kindly sent down for exhibition by Mr. Charlesworth. Among the most striking were teeth of different genera of sharks, scales of fish and reptiles, coprolites, ear-bones of porpoise, dental palates and tusk of walrus.

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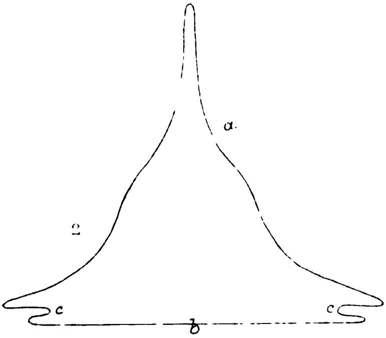
* Secretaries of Societies will greatly oblige us by writing their report legibly —especially by printing the technical terms thus: Hydra—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—ED. 'M. M. J.'



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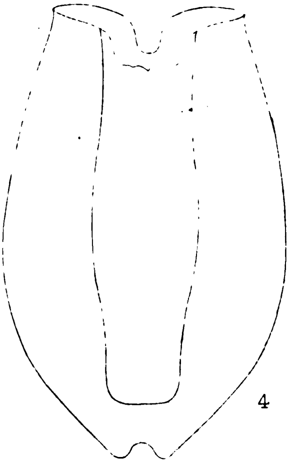

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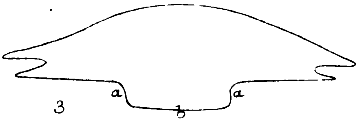

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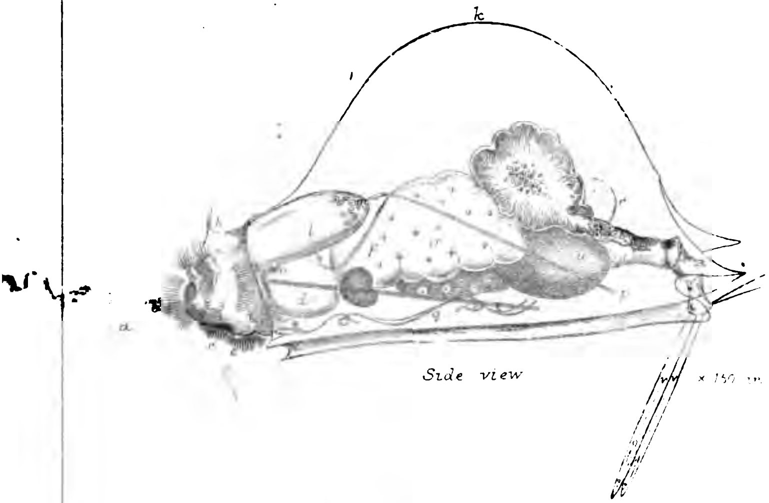
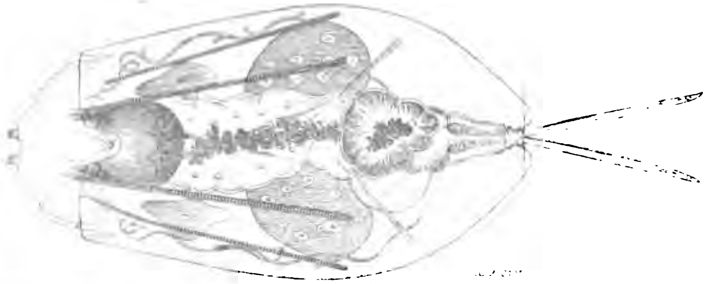


Side view



Dorsal view

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1872.

I.—*On Euchlanis triquetra and E. dilatata.*

By C. T. Hudson, LL.D.

Plate XXVIII.

The genus *Euchlanis* belongs to one of the most fascinating and perplexing families of the Rotifers. The members of its various species are large, brilliantly transparent, elegant in form, and remarkable in structure; so that it is impossible to find more beautiful objects for the microscope than these living atoms. But, on the other hand, the species have been badly described and wofully confounded together; several of the genera are hopelessly involved one with another, and even the family itself is threatened with extinction by a more modern classification than that of Ehrenberg.

Indeed Ehrenberg's system has so broken down under the weight of new discoveries, that I need make no apology for setting it aside while attempting to show how the rotifers may be roughly grouped, until some really good system of classification has been hit upon.

The most obvious difference of habit among these creatures is that some spend the greater part of their lives attached to a plant or other foreign body, while the rest generally move freely about from one spot to another. This difference would at once separate the Tube-formers from the Free-swimmers.

The "*Tube-formers*" subdivide at once into two groups, *viz.* 1st, those in which the mouth is symmetrically situated with respect to the body, lying in its longitudinal axis, or the *Floscules;* and 2nd, those in which the mouth is asymmetrically placed, lying on the ventral surface, or the *Melicertans.*

The Free-swimmers break up into two well-marked groups; 1st, those that both swim and creep like a leech, or the *Philodines;* and 2nd, those that only swim; and these latter again subdivide into a loricated and an illoricated family, to wit, the *Hydatines* and *Brachioneans.*

Now, according to the above classification, Ehrenberg's family of the *Euchlanidota* would disappear; for as all its species consist of free-swimming loricated rotifers, it would rank with the *Brachionæa.*

But the reasons that induced Ehrenberg to make a separate family of the *Euchlanidota* are worth the considering, as they will naturally lead me to speak of some of the curious peculiarities of the family as a whole, and especially of the two species of the genus *Euchlanis*, which more properly form the subject of this paper.

Ehrenberg's imperfect instrument led him to believe that he could divide the trochal disks of the rotifers into four well-marked groups, *viz.* 1st, those with a single ciliated edge; 2nd, those with many ciliated protruberances; 3rd, those with two equal ciliated circlets; and the first was again subdivided into single ciliary edges with some of the cilia wanting, and perfect single ciliary edges.

Now it may be said briefly that the whole of this scheme is worthless. The modern instruments have shown not only that the trochal disks are often of a highly-complicated structure, and that they possess subsidiary lines of minute cilia, of the existence of which Ehrenberg was quite unaware; but also that the forms of the disks pass so gradually from the one into the other that it is hopeless to attempt to ground any system of classification upon them. I may add to this also that it is the most difficult thing in the world to determine in almost any rotifer (except the Tube-formers) what is the real form of the trochal disk. In confirmation of this statement I have only to call attention to the drawings of even such excellent observers as Leydig, Cohn, and Gosse, in which very frequently a row of lines drawn at random follows the outline of the head of the rotifer without the least attempt having been made to depict the real arrangement of the cilia.

Now, according to Ehrenberg's notions, the *Euchlanidota* had the trochal disk of the *Hydatinea,* while themselves encased in loricæ like the *Brachionea:* so he formed them into an independent family, and at the same time pointed out that the great majority of the typical genus *Euchlanis* had a lorica slit longitudinally down the ventral surface.

As to the trochal disk of *Euchlanis,* I have spent no little time and patience in endeavouring to understand it, and I *think* that the disk of *Euchlanis triquetra* is correctly given at (*a*), Fig. 1, Plate XXVIII. If so, it does not much resemble that of *Hydatina,* in which there are, broadly speaking, two horse-shoe shaped rows of cilia one within the other.

If anyone would like to test at once the quality of his temper and his microscope, I strongly recommend him to study the trochal disk of one of these creatures from a living specimen, and with the full determination of thoroughly understanding it.

What a severe trial it will prove to both will be readily seen when it is understood that *Euchlanis* is incessantly in motion, now rolling over as it swims, now catching hold with its pincers, and jerking itself backwards and forwards; and that the instant it is held with the compressorium it begins to thrust its head in and out of its lorica, and continues to do so, without intermission, till it is released.

It is useless, of course, to use high powers on a rapidly-moving object, and yet with powers less than ¼ inch very little can be made out. The most successful method of attacking the puzzle is the employing of the dark-field illumination, especially that with Wenham's paraboloid.

It will be seen from Fig. 1 that if I have rightly interpreted the disk, the cilia on *Euchlanis triquetra* are divided into several groups, two at the sides, like whiskers (*b*, *b*), two parallel rows (*c*, *c*) running down to the mastax (*d*), and performing the office of conducting to the mouth and jaws the food brought by the coarser cilia.

There is another row (*e*) along the forehead, while from the disk itself spring up four papillæ (*f*, *f*), (*g*, *g*), the former of which have curved motionless setæ on them, while the latter (*g*, *g*) are evidently perforated tubes, and probably contain some tactile organ, which, however, I have never seen protruded. There is an obvious tactile organ (*h*) on the medial dorsal line and just above the disk.

Before I describe the internal organs I will take the lorica, which Ehrenberg affirms to be slit underneath longitudinally right down the middle of the ventral surface. Here I am quite positive that the great naturalist is in error; the peculiarity he describes does not exist, while he omits one quite as striking.

If *Euchlanis triquetra* be carefully watched with the dark-field illumination, the flashing of the light from its extremely transparent but highly reflecting lorica will reveal its true form. It will be best understood from Fig. 2, which is a cross-section of it through the middle of the back, *viz.* at the point (*k*) in Fig. 1.

The lorica is something like a glass dish-cover, rising into a very high ridge, and placed on a dish which it overlaps.

Fig. 2 shows the upper portion or cover (*a*) and the lower flat plate or dish (*b*), with a connecting incurved rim (*c*) lying between; though whether this portion be chitinous, like the lorica itself, or only a soft membrane, I have been unable to discover. That there is a connection may be easily shown by drying the animal on a slip of glass, when on again dropping water on it, the connecting portion may be seen to bulge out.

Fig. 3 is a similar cross-section of *Euchlanis dilatata;* which animal also, in my opinion, has no medial division of the under portion of the lorica: it *has*, however, a raised portion (*a*, *a*), the edges of which, when the animal is held by the compressorium, give rise to two sharp lines, which appear to be the bounding lines of the under surface of the lorica, and exactly give the impression of there being a naked gap at (*b*), leaving the body unprotected. This is better seen in Fig. 4.

To return to *Euchlanis triquetra.* Of its internal organs, its enormous brain (*l*) claims the first notice. Anyone unacquainted with the subject may well smile at the idea of crediting such an atom with such a brain; but the reasons which lead those who study the rotifers to say that the organ is a nervous centre are, I think, convincing. To lessen the marvel I may mention that the contents of the brain sac (*l*) seem to be of a very fluid and transparent nature, with the exception of the hinder portion (*m*), which appears to be nucleated and more dense.

Immediately under the brain is the red eye-speck (*n*) and the mastax (*d*), while above it and close to it is the tactile organ (*h*); this arrangement is pretty general among the rotifers.

The passage through the mastax leads by a short tube to the stomach (*o*), which is generally dotted over with oil-globules, and often in quicunx fashion.

Two muscles (*p*, *p*) cross the surface of the stomach, imbedded in its surface, and constantly twitching it from side to side. The stomach is usually subdivided into an upper and lower portion, as in the figure; the lower portion being densely ciliated. But this subdivision is not permanent in the same individual.

There are the usual convoluted tubes (*q*) on either side of the body, bearing on an average four or five vibratile tags on each side; and at (*r*) is the contractile vesicle, into which the tubes open, and from which a discharge is kept up every ten or twelve seconds into the vent.

The ovary (*s*), containing the embryos (*t*, *t*) and the full-developed egg (*u*), opens also into the same place.

I have only to add that the pincers (*v*, *v*) exude, apparently, a viscid substance, like that of a spider's web, by means of which the animal adheres to any substance it pleases. I have never seen them *pinch* anything, but I have seen them adhere by their extreme tips to the glass; and I think I have on more than one occasion seen traces of this viscid secretion on the glass itself: the power of secreting such a fluid is, I believe, common to the majority of the rotifers.

The figure of *Euchlanis dilatata* (Fig. 5) is so like that of *E. triquetra*, that it requires no further explanation.

II.—*On some Methods for preparing the Tissues of the Frog Tadpole's Tail.*

By R. L. Maddox, M.D., Hon. Fel. R.M.S.

Of the various methods employed by me lately to try and render more or less transparent some of the tissues of the common frog tadpole's tail without inducing a too great granularity, and yet give distinct views of the relations and paths of the different sets of nerves, cutaneous and deeper seated, I beg to offer the following, thinking the plans may be of interest to readers of the Journal, and believing they embrace some novelty in the treatment. The tissues are not stripped, hence the relations are not destroyed; but the epidermis is removed so as to permit a clearer view into the thin parts of the tail, in fact completely through the textures. It is not worth while to do more than briefly recount some of the numerous methods, and those which have yielded excellent results, more in detail.

Methods 1, 2, and 3.—After removing the epidermis, as described farther on, chloride of gold and chloride of cobalt solutions mixed in various strengths and various ways were tried.

Methods No. 4 and 5.—Chromic acid solution followed by tincture of logwood and chloride of zinc solution; also the chloride of zinc replaced by chloride of tin solution (these two reagents having been employed by me in 1868, when working on the structure of the papillæ of the frog's tongue, and found to beautifully stain the nerves of a blue or blue-violet colour); and these plans used in different ways more or less successfully on the tadpole's tail, but did not equal the methods following.

No. 6 Plan.—Living specimens, also tadpoles that had been dead at least for twelve hours, were placed in chromic acid solution, 3 drams (strength $\frac{1}{4}$ per cent.) with 20 drops of spirit of sweet nitre, for five minutes or a little longer; then washed in pure water thrice, changed for ten minutes; now immersed in a few drams of ammoniated water (strength 4 drops of the strong liquor of ammonia to 3 drams of pure water) for about three minutes or until the epidermic epithelium was softened into a sort of mucoid film, then *quickly* removed into pure water in a Wedgwood evaporating dish, and gently brushed under water with a soft long camel-hair pencil; now rewashed for ten minutes, changing the water three or four times, and replaced in fresh chromic acid and sweet nitre mixture for twenty minutes; then rewashed in several changes of pure water for ten minutes, and placed in diluted tincture of logwood for five minutes (strong tincture diluted with pure water until of a golden or sherry colour); again well washed and placed for three minutes in the usual iron developer with acetic acid employed for developing photographic negatives; again washed with several changes of water, and mounted in a nearly saturated solution of acetate of potash slightly acid with acetic acid.

This plan furnished beautiful results. The reason why the iron developer was used arose from finding that if the specimens after the logwood tincture were washed with our ordinary well water, which contains a little iron, they were greyer in tint than if washed in pure water.

The same plan was modified by placing twice alternately after removal of the epithelium, into the chromic acid and sweet nitre mixture and diluted logwood tincture, without using the iron developer, and which gave fair specimens.

No. 7 Method.—The same as No. 6 plan, only keeping the tadpoles in the chromic acid and sweet nitre mixture for forty minutes before removing the epithelium by the ammoniated water, and after its final removal very gently by the brush, rewashing and placing the specimens in the tincture of logwood first for five minutes; then well washing and replacing in fresh chromic acid and sweet nitre for ten minutes; again washing and mounting in the acetate of potash, as above. The specimens did not quite equal No. 6 plan.

No. 8 Method.—Living tadpoles were placed for fifteen minutes in the chromic acid mixture, and well washed; then the epithelium softened by the ammoniated water and removed under pure water, as above described; again well washed and placed in "Mordan's purple ink" diluted with two-thirds water for ten minutes (ink supposed to be made from aniline), well washed in water weakly acidified with acetic acid and mounted in acetate potash solution.

This also gave beautiful preparations.

The attempt to hurry the effects of the reagents by increasing their strength and diminishing the times did not yield equal results. The mixtures and the ammoniated water to be used fresh each time. Dead specimens, in which the fine nerve threads might be supposed to have considerably changed, did not under the microscope appear to have suffered in any way, save that the epithelium could be removed after a very *brief immersion* with ammoniated water.

This plan may be found useful for some living structures, or such as are only recently dead. Instead of stripping the sides from each other, the central muscular portion of the tail, snipped off *after preparation*, is carefully removed by scissors under water, then the thin portions lifted out on a thin silver spatula, drained from the edge by blotting-paper, and gently pushed off into the mounting medium on the slide. If steel instruments be used they very quickly tarnish by the acetate potash and acetic acid, get slightly roughened, the delicate specimens cling to the surfaces, often become torn or doubled over, and thus they are somewhat difficult to spread out again. Glycerine was tried for the mounting medium, but the appearances were more natural in the acetate potash medium. More than a hundred specimens were treated in various ways, too long to enumerate here, the preference being given to the plans fully detailed.

III.—*On the Employment of Coloured Glass in Microscopy.*

By M. Mouchet.

I believe that the use of coloured glasses in microscopical studies may facilitate the examination of many objects which would otherwise remain drowned in a flood of light; I believe also that the use of them must remarkably spare the eyesight. It is many years since Ch. Chevalier first employed tinted glasses, which he placed above his lenses in order to temper the intensity of the light. By the use of those glasses, or of silvered glass, sunlight may be made use of, and may thus replace all the other systems of illumination which are intended to supply the brightness that powerful objectives tend to destroy. Now, what position should these different glasses occupy? We can count four: before the mirror; under the object; between the objective and the eye-piece; finally, above the eye-piece itself. To place them before the mirror necessitates the employment of glasses of a certain size; to place them under the object increases the thickness of the plate and prevents a very oblique light from passing through; to fix them in the body of the instrument causes trouble and a loss of time, for the microscope must be taken asunder. Above the eye-piece appears to me the most suitable place. For this purpose, I have constructed an apparatus, consisting of a kind of revolving diaphragm pierced by nine openings, each one centimètre in diameter, and placed exactly opposite the eye-piece. Seven of these openings are fitted with coloured glasses corresponding to the colours of the solar spectrum; another with a silvered glass, according to Foucault's process; and the ninth with a very finely roughed glass, which may be removed at will, and thus leave one opening free. This revolving diaphragm is attached to a ring placed on the tube which receives the eye-piece, and is fixed to the tube by means of a *vis de serrage*, as in certain "*chambres claires*" (cameræ lucidæ ?). The apparatus has some analogy to that which Mr. Collins calls "light corrector," and which he places on the stage of the microscope.

It is evident that when coloured glass, as, for example, cobalt blue glass, is used, we have not a pure monochromatic light; but this glass replaces, if not with advantage, at least without inconvenience, the vessel of ammoniacal sulphate of copper patronized by Count Castracane.

At first, I wished to construct, without any modification, Mr. Collins's accessory plate, which for the optical effect may be placed in the microscope more advantageously than my apparatus; but to keep the chief plate of the least possible thickness is so powerful a consideration, that I decided on adopting the plan which I have just described.

In order to substitute one glass for another when examining an object, I have no need, as with Mr. Collins's arrangement, to change my position; and I can observe, without losing sight of the object, what is the most favourable colour for the perception of certain details peculiar to it.

However, I am not yet sufficiently acquainted with the advantages to be derived from those glasses. They will, perhaps, become useful auxiliaries to micrographists, by enabling them to make more frequent use of solar light, without experiencing the least fatigue.

IV.—*Note on Silicious Incrustations from the Geysers of the Yellowstone River, Wyoming Territory.*

By Prof. Arthur Mead Edwards, U.S.A.

The specimens are of two characters. First, we have a mass of white stony material, arborescent in appearance and looking as if it were some species of vegetation, as a moss, converted into a mineral. The others are rounded masses, smooth on the exterior, white, and evidently consisting of the same substance as the arborescent specimen. These were given to me by Mr. P. T. Austen, of the School of Mines, who has made a chemical analysis of them with the following result:—

SiO_2	87·23
Fe_2O_3 and Al_2O_3	1·13
CaO	·55
MgO	·46
PO_5	1·48
HO	9·09
	99·94

For my own part, being interested in the subject of the mode of formation of concretions of the character of these rounded masses, I determined to sectionize one of them and examine it by means of the microscope, to ascertain, if possible, what kind of nucleus it had. This matter is one of importance on account of its bearing upon the mode of formation of pearls and the rounded masses which, united together, constitute the dentine of teeth, and which in that substance are often so loosely adherent one to the other as to leave cavities known as "interglobular spaces," and the existence of which is by some observers denied, the appearance of them in tooth sections being considered "optical illusions." I now exhibit one of these silicious balls thus sectionized or cut down to a thin slice which runs directly through the centre of the mass, and viewing it by means of a pocket-lens will show plainly how it has been formed. At first, the silicious matter has been deposited from solution upon the sides of the geyser in the vegetation, like masses shown. Although they seem pretty strong, yet they are liable, on account of their porous nature, to be broken so that at some time a small mass has been broken off and rolled and tossed about in the basin until it has gradually come to be covered over with a smooth coating of the same material. Thereafter it has rested for awhile somewhere within the basin, so that it has been again encrusted with rough material, and dropping down into the tube of the geyser, has again been thrown about until a smooth coat has formed over the rough one, and so on alternately rough and smooth coats have been deposited. It will be seen that the specimen I exhibit has several of these alternate compact and porous coatings, so that to the naked eye it almost seems to have an organic structure.

These specimens were brought home by the exploring party under the command of Dr. F. V. Hayden, United States' geologist, who has for some time been engaged in the examination of the Yellowstone Park, in Wyoming Territory, under the direction of the Department of the Interior; the survey being known as the United States' survey of the Territories. Dr. Hayden has brought home many specimens of the Diatomaceæ from the geysers, as well as several earths containing the silicious remains of those organisms, and these he has put into my hands for examination and report. Further collections will be made, and the results of the examinations published hereafter. In the meantime, Dr. Hayden has kindly consented to my making known here and elsewhere the results of my examinations, which I shall do from time to time. —*Read before the Lyceum of Natural History, New York, March* 25, 1872. Sent to 'M. M. J.' by the author.

V.—*On Angular Aperture of Immersion Objectives.*

By Robert B. Tolles, Boston, U.S.A.

The following table gives measurements of angle of aperture for several objectives—taken indiscriminately, not selected for the purpose—in balsam, in water, and in air. Several of them were made years since:—

Objectives.	Angle in Air.	Angle in Water.	Angle in Balsam.	
$\frac{1}{18}$" Single Front	170°	100°	81° (+)	Sun.
$\frac{1}{18}$ Single Front	170°	120°	87°	"
$\frac{1}{18}$ Single Front	170° (+)	110°	83°	"
$\frac{1}{16}$ Compound Front	147°	97°	82½°	"
$\frac{1}{10}$ Compound Front	175°	117°	93° (+)	"
$\frac{1}{10}$ Single Front	175° (+)	113°	95°	Lamp.
$\frac{1}{6}$ Compound Front	178°	120°	97°	Sun.
$\frac{1}{5}$ Compound Front	155°	106°	88°	"
$\frac{1}{8}$ Four Systems	175°	127°	110°	"

This table of measurements of angle in balsam, &c., furnishes proof enough of the correctness of my position so positively denied by Mr. Wenham, first in 'Mon. Mic. Jour.,' No. xxxii., p. 84; again, *ibid.*, No. xxxvi., p. 292; and thirdly, No. xl., p. 272, of the same. The criticism bestowed in these articles induced the measurements. Still, if nothing of attempt to deny the significance of that first experiment as described in this Journal, No. xxxii., p. 36, had appeared, the above results of actual measurements would have been interesting as data of practical angle of "immersion" objectives when water or any medium of ref. index not higher than 1·549 that of balsam is used. But *somebody is mistaken*, and I must necessarily be "personal" in pointing out how I am not, and others are, in respect to what I have set forth on angular aperture. In the first place all agree that in the case of a balsam-mounted or dry-mounted object-slide, if of common glass in use, the light incident at the first outside surface of that slide at the extremest obliquity will be so bent in entering the glass as to make the angle of the pencil after this first refraction less than 82°, the extreme rays of the pencil having approached the glass at an incidence of more than 89°, and the pencil angle more than 178°. Not to include the case of a slide of dry-mounted objects, it is safe to state that with whatever kind of *objective* used, when the slide and covering glass are by balsam between fused into one (nearly) homogeneous transparent piece, this first refraction limits the angle of transmitted pencil to less than 82°. The barrier evidently is at the first refracting surface of the slide. The little (or large) hemispherical lens cemented to the outer surface of the slide* gives the case I first gave substantially. It is evident that as the light, say a ray for simplicity, is carried from the axis round to the extremest obliquity, the path of the ray will be a straight line to the centre of curvature of the plano-convex lens. This figure illustrates plainly that the *hemispherical* lens gives access for the largest angular pencil to the balsam-mounted object, say 179°. Here the lens is not fully a hemisphere, but *nearly* of the requisite thickness. The diagram previously given† shows the full pencil admitted.

In giving my first note and illustration I assumed as evident enough from what *had already appeared* that with a balsam-mounted object-slide an immersion objective would have more angle for rays *radiant from the object* in the balsam than a dry objective could have. But it is denied. And yet it is safe to say that with *air* between the immersion-front and slide-cover, the latter being of glass commonly used, the "well-settled laws of refraction" limit the pencil, *emergent*, at the outside surface of the "cover" to 82°,—*within* the glass, *before* emergence; but with water between the cover and front surface of the immersion objective it would be *larger* according to the difference between refraction of covering-glass in water and covering-glass in air. Whether the immersion objective would be competent to transmit the whole angular pencil or not is another question. That it could be made to do so did not seem to me to admit of doubt. Mr. Wenham's $\frac{1}{12}''$ 170° air angle, 100° water angle, probably would not transmit or "collect" to exceed 82°. "Mr. Tolles is" not "unable to perceive this." All of my own save one, so far as tried, *do* take up more than 82° when immersed in balsam. That exceptional one has the same angle in air and in water as given by him as pertaining to his $\frac{1}{12}$ in. When I "seized" upon his own experiment as proof that my experiment was valid, it was only because his result agreed exactly with my result—objective 170° in air, 100° in water. *All* that his or my experiment proved was that more than 82° could *get out* of the balsam through the cover into *water*, unless I include that my plan showed that in air all but 82° of pencil was *turned back*, which his did not! Now again, "100° is surely more than 82°"! Perhaps Mr. Wenham "is unable to perceive this, and so let it remain."

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* See 'M. M. J.,' No. xxxix., p. 117, Fig. 3.

† 'M. M. J.' for Nov. 1871.

For persons not "familiar with optical science," and for others, Mr. Wenham's tank method furnishes a ready means of *test* of angle in balsam, thus ascertaining the degree of fitness of any objective for viewing objects balsam-mounted. It proves, moreover, by simple experiment what many persons would not take the trouble to study out from a diagram and description, and the *experiment* is conclusive, at all events as to the point at issue between Mr. Wenham and myself. The measurements given at the head of this article for angle of objectives in water and in balsam were made strictly according to that tank method as contributed by him, only sunlight was used in most cases. The differences in the results are plainly referable to differences in the objectives as to plan of construction. There is no need to set forth the manner in which the result is produced in one case or another; the fact remains of comparatively large angle for immersion objectives over what is possible for "dry." In proof that image-forming rays were concerned, I name the fact that every objective recorded in the table *readily shows the true test, A. pellucida, by uncondensed light of a common petroleum lamp flame.* In this connection let it be noted that most of the objectives in the list have the *compound front* of crown and flint glass, while some of them of equal angle have the single lens front.

The $\frac{1}{5}''$ of 110° *in balsam* has four systems substantially on the plan figured in 'Mon. Mic. Jour.,' No. xxxix., p. 127, Fig. 1. It is easy to carry the angle on this plan much beyond 110°, but that breadth of angle, anything above 82° of course, is enough to dispose effectually of this extraordinary assumption and *defy.* "I challenge . . . . . anyone to get, through the object-glass with immersion front, a greater angle, or any portion of the extraneous rays that would in the other case be totally reflected, as no object-glass can collect image-forming rays beyond that limit."*

One word about Fig. 1 (dotted lines) and Fig. 2, p. 117, No. xxxix., this Journal. Mr. Wenham says they are grossly erroneous. He is at fault. In the text, this in qualification and explanation, is stated as follows:—"What is intended is to increase the refraction of the front by sharper convexity or higher refractive material or *both* to the extent necessary to make up for the diminution at the front surface which takes place according to the refractive power of the medium flowed in" between the front and covering glass. Higher refractive material involves *refraction* at the front plane surface, of *course and necessity.*

As to the practicability of the *first case* illustrated by the same figure, involving *four* systems in the objective, that seems to be admitted. It is not denied. I understand Mr. Wenham to admit it, in effect, in two of his three replies. The $\frac{1}{5}''$ in the table, of balsam angle 110°, is, as before stated, made on that plan, but not strictly according to what the figure represents, for *that* is an *extreme* case, to help apprehension, and the refraction is all accomplished by the three inner systems. In this $\frac{1}{8}''$ (of the table) the front lens is *thin* and has *considerable* action to bend the rays towards the axis, and the objective has a good range of working distance. To get the highest *practical* balsam-angle on this plan, of course the action of the two lenses at the front of the objective is so apportioned between the two as to favour convenience in use as well as efficiency. In this diagram, moreover, the three inner systems are identical with those of Mr. Wenham's $\frac{1}{8}$, as he represented them, and I thus limited the case, and *all the question,* to the added fourth lens. Therein was the reason his diagram was copied. *That part* he would not impeach. *I never used* such a set of combinations. Would not select it as choice for any purpose.

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* Mr. Wenham, in 'M. M. J.,' No. xxvii., p. 118.

In conclusion, one point may as well be noted, as of some practical importance, *viz.* that with a *water* immersion objective in use, a water immersion condenser of the same balsam angle will give closely the full dimension of *illuminating pencil* that *can* reach the water-immersed objective above, and be transmitted by it. About 125° balsam angle, for the condenser would cover *all* cases of use of *water* immersion objectives. I pointedly suggested this in my first communication.

BOSTON, *July* 23, 1872.

VI.—*The Minute Anatomy of Two Cases of Cancer.*

By Assist.-Surgeon J. J. WOODWARD.

PLATE XXIX., from the Author's Photographs.

OPPORTUNITIES for the study of the histology of morbid growths frequently occur at the Army Medical Museum. Specimens are sent, not merely by medical officers of the army, but also by civil practitioners. It is often possible to examine these growths while quite fresh, and thus to make observations which aid in the interpretation of the appearances found in those which are not received until they have been hardened by immersion in alcohol or other reagents. By methods described in former reports, it is possible to preserve thin sections of very many of these growths in a permanent form for examination with the microscope, and a large collection of such preparations has already been formed.

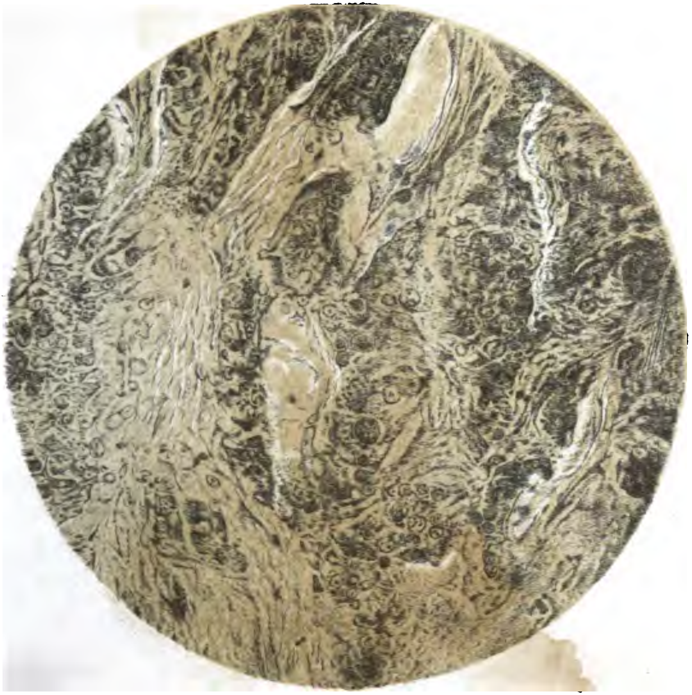
These preparations are frequently studied by medical officers and others who visit the Museum for that purpose, but they would be rendered more generally useful if it were practicable to publish from time to time brief descriptions of the more important cases, accompanied by suitable illustrations.

The usual methods of representing such microscopical preparations by etchings on copper, lithographs, or woodcuts, are open to many objections. Especially it may be indicated that the production of the drawings which must serve as the basis of such representations is laborious, and consumes too much valuable time if the investigator himself endeavours to make them; and if the work is entrusted to a professional artist the essential features of the image are very generally imperfectly shown, or altogether misrepresent the original. Even if the microscopist himself has the leisure and the skill to make his own drawings, the finished work will too often represent rather his theoretical views than the severe facts of nature, for in any case the artist finds it impossible to represent all that he sees in the microscope, and only attempts to reproduce so much as he conceives to be of importance.

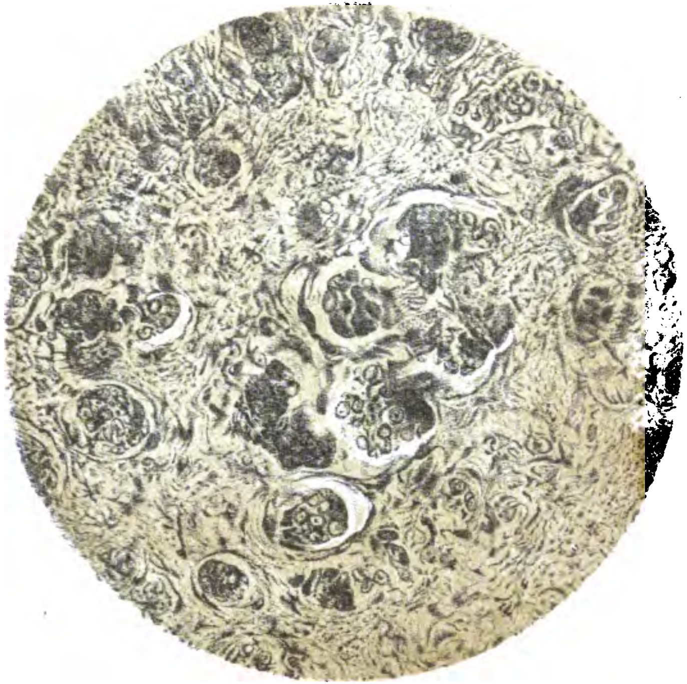
Much more truthful representations can now be made by the process of Photo-micrography as improved by my experiments; but the cost of making silver prints, the fact that they are not permanent, and the circumstance that they require to be mounted on tolerably stiff cardboard, prevent them from being generally available as book illustrations, especially where editions of several hundred copies or more are desired. Under these circumstances I note with pleasure the rapid improvements lately made in the direction of photo-mechanical processes of various kinds, having for their object the reproduction of photographic negatives in inks of a permanent character. These processes are indeed as yet far from perfect, but several of them have already reached such a degree of excellence as to be very suitable for the reproduction of photographic negatives of microscopical images.

In the United States two such methods may be said to be in successful operation, *viz.* the Woodbury process, and the Albertype process. A detailed account of these processes would be out of place here, but a few brief remarks are required.

The Woodbury method consists essentially in the production from the negative, by the action of light, of a relief surface of gelatine, from which a metal "intaglio" is produced by pressure. In this a series of gelatine films coloured by any suitable permanent pigment are formed by mechanical means, and these constitute the prints. Like silver pictures the Woodbury prints require to be mounted on sheets of cardboard or stiff paper, and this of course adds to the expense. Moreover, if the mount is thick it is ill adapted to binding. Of late, however, films of great flexibility have been produced, which are supported on mounts not much



Mammary Cancer.



Cancer of the Liver.

The figures are reduced ⅓ from Col^{l} Woodwards photographs.

West & Co. imp.

stiffer than ordinary plate paper, and therefore quite suitable for book purposes.

In the United States this process is employed by the American Photo-relief Printing Company, No. 1002, Arch Street, Philadelphia, under the supervision of Mr. John Carbutt, who has reproduced three of my negatives of microscopic objects, *viz.* a photograph of a section of ovary magnified 400 diameters, of which five hundred copies were printed, mounted on stiff cardboard, and used among the illustrations of my report on photographing histological preparations by sunlight; a photograph of *Amphipleura pellucida,* in illustration of a paper published in the 'American Naturalist' for April, 1872, and a photograph of two frustules of *Triceratium fimbriatum,* in illustration of a paper published in the Chicago 'Lens' for April, 1872. Large editions of the last two prints were struck off for the journals named. They were mounted on plate paper.

In quality these prints were almost fully equal to fine silver prints, which they closely resembled, but the editions were much more uniform than would have been possible had silver prints been used. The cost, moreover, was considerably less than that of silver prints, and the negatives were returned to me wholly uninjured, while they would have been much damaged if not quite destroyed had an equal number of silver prints been made from them.

The Albertype process is employed in New York by the Photo-Plate Printing Company, under the supervision of Mr. E. Bierstadt, No. 932, Broadway. In this process a printing surface (not a relief) is produced on a gelatine film by the action of light, through the negative, on certain chemicals contained in the film. The surface thus produced, when properly inked, yields in the press an impression on paper, in which the details of the original negative are very well preserved. The prints may be made on either plain or enamelled paper; in the latter case they closely resemble silver prints on albumen paper, in the former they are like silver prints on plain paper. In either case they give a clean white margin, and therefore do not require to be mounted as Woodbury prints do, but are at once ready for binding. Moreover, proper descriptive lettering is readily added to the plate. If suitable ink is employed the prints are as permanent as lithographs or other engravings.

Two of my negatives were placed in Mr. Bierstadt's hands for reproduction, and the illustrations of the present paper are the results. These plates very fairly represent the original negatives. One of them is on plain, one on enamelled paper. The latter more closely resembles an albumen print from the negative than the former does, but the enamelled paper has a certain glare, and I observe that it easily cracks if crumpled. It is a question how far these disadvantages counterbalance its advantages. I place the two prints side by side, and should be glad to hear comments on their respective merits.

I may add that the prints of the edition of these two plates are not so uniform as the Woodbury prints above alluded to. Some are much darker, some much lighter than the standard desired. I cannot say, however, that this irregularity is as great as is usually the case with silver prints. In cost the Albertype prints, paper included, were rather cheaper than the unmounted Woodbury prints.

In selecting a subject for this report, which it is to be hoped may be regarded as the first of a series of short essays on morbid growths, I determined upon a case of multiple carcinoma, consecutive to the ablation of a primary cancer of the female breast. With this I have associated a brief description of a primary mammary tumour of similar structure.

The microscopical anatomy of these growths appeared of interest in connection with the recent study of Koester on the development of carcinoma and sarcoma.*

Koester has been led to the opinion that carcinomatous growths very frequently have their origin in a proliferation of the endothelium of the lymphatics of the parts involved, and that this transformation plays probably a great part in the development of most cancers and sarcomas. His work, so far as published, is confined to the examination of epithelial cancer of the skin, and alveolar colloid of the stomach (alveolarer Gallertkrebs des Magens), and he has postponed for future parts the detailed examination of other varieties. His views have been received with much favour in various quarters; I may especially mention Rindfleisch, as having given prominence to them in the last edition of his text-book.† I shall have occasion to refer further to the views of Koester in the sequel.

The following is briefly the history of the case selected for consideration: —

Case. Martha J. H——, a widow, forty-three years old, who had resided for twenty years in the District of Columbia, most of the time in East and South Washington, where she had frequently suffered from intermittent fever, first noticed a tumour in her right breast in June, 1865. The whole mammary gland was removed November 23, 1866, by Professor Johnson Eliot. The tumour, which had attained the size of a hen's egg, was hard, uneven, and somewhat painful. No microscopical examination was made.

The patient speedily recovered from the operation, but never regained full constitutional vigour. In March, 1867, her health began to give way, and during April her strength failed rapidly, so that she was confined to her bed. Loss of appetite and vomiting were prominent symptoms.

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* 'Die Entwicklung der Carcinome und Sarcome,' von Dr. Karl Koester Erste Abtheilung. Wurzburg, 1869.

† 'A Text-book of Pathological Histology.' By Dr. Edward Rindfleisch. American translation. Philadelphia, Lindsay and Blakiston, 1872.

After a time there was some temporary improvement, the vomiting ceased and her appetite returned, becoming in fact excessive. A tumour now appeared in the left breast, and the attention of her medical attendant was drawn to a decided enlargement of the spleen. No great pain was complained of, but the patient emaciated constantly, lost strength progressively, and died March 22, 1868, the tumour in the left breast not having yet ulcerated.

Autopsy by Dr. D. S. Lamb. The body was extremely emaciated. There was a small, hard, nodulated tumour of the left breast. The lymphatic glands of the axilla, neck, and groin were enlarged and indurated. The thoracic and abdominal cavities were filled with straw-coloured serum. The lungs were coated with lymph, but there were no adhesions. The lower lobe of the left lung was congested and somewhat œdematous. There was some little serum in the pericardium. The heart and pericardium, liver, spleen, and internal organs of generation were removed, together with the tumour of the breast, and sent to the Army Medical Museum for study, by Dr. George Rice, the attending physician.

These specimens are preserved in the Medical Section. The following is a description of their characters:—

No. 955, Medical Section. The left mammary gland, which is transformed into an oval, flat, somewhat nodulated scirrhous tumour, three and a half inches long by two and a half broad, and about an inch thick in the centre.

In thin sections of this tumour individual lacteal alveoli and ducts, stuffed with large cells, derived evidently from the glandular epithelium, were but sparsely scattered, and the greater portion of the gland was replaced by a peculiar scirrhous tissue, in which its scanty atrophied remains were imbedded. This peculiar tissue consisted of a firm, delicately-fibrillated connective-tissue stroma, containing numerous small cells and many fine elastic fibres, and of a plexus of irregular, varicose, nucleated cylinders of protoplasm, corresponding in many respects to the "cell cylinders" of Koester, which intimately interlaced with the connective-tissue stroma, filling all its meshes except those occupied by the vessels and the atrophied remainder of the gland. These nucleated cylinders did not appear to be composed of separate cells. They seemed rather to consist of a granular protoplasm, in which innumerable nuclei, $\frac{1}{2500}$th to $\frac{1}{2000}$th of an inch in long diameter, or even larger, were imbedded side by side, without any evidence of limiting cell-walls separating the protoplasm surrounding each nucleus from that belonging to its neighbours. On scraping or teasing the sections, moreover, the nuclei escaped abundantly into the "juice" in a quite naked condition. Very often, however, some of the nuclei appeared in the "juice" thus obtained surrounded by a more or less irregular mass of protoplasm, resembling thus "the typical cancer cells" of the older writers.

The nucleated cylinders usually varied from the $\frac{1}{1000}$th to the $\frac{1}{500}$th of an inch in diameter, but in some parts attained still greater dimensions. In places where the section passed transversely through the cylinders they appeared as round or somewhat oval masses of protoplasm, stuffed with nuclei, which might readily pass for the "mother-cells" of the books. In glycerine and balsam preparations the protoplasm of the cylinders was often shrunk away from the connective-tissue stroma, the margin of which being clear and transparent might almost be taken for a limiting membrane.

The nuclei of the cylinders were transparent or but slightly granular when fresh, and usually contained a single large nucleolus. In the balsam-mounted sections they appeared more decidedly granular. Elongated nuclei with two nucleoli and other indications of commencing division were but rarely observed. The nuclei of the connective-tissue stroma were generally rather longer than those of the cylinders, but much narrower. Occasionally their position was occupied by little rows of two, four, or more nuclei, which might be supposed to be the progeny of the connective-tissue nuclei. Such rows were generally surrounded by a small quantity of protoplasm.

Sections taken from the peripheral portions of the tumour generally presented a certain amount of more or less altered adipose tissue. The quantity of the matrix between the fat-cells was much increased, so that instead of the individual cells of each fat-lobule being apparently in contact, they were separated to a greater or less extent (often the $\frac{1}{2000}$th of an inch or more), by a delicately-fibrillated matrix, in which were imbedded many oval nuclei which were both larger and more numerous than normal. The connective-tissue septæ between the individual fat-lobules were also greatly thickened, and in many places contained cell cylinders quite like those of the body growth. (See Microscopical Section, Nos. 4615 to 4631.)

No. 958, Medical Section. A portion of the right lobe of the liver. The liver was nearly normal in size, but presented on section numerous little hard whitish nodules, varying from the size of a pin-head to that of a pea, or even smaller. These were invariably situated in the course of the portal vessels, *i. e.* in the interlobular spaces. In structure they closely approximated the scirrhous tissue of the breast tumour as above described, simply the meshes of the plexus of nucleated cylinders were less elongated, and in the sections, therefore, these more frequently appeared as round or oval forms (the cylinders having been cut more or less obliquely), and less frequently as continuous cylinders of some length, such as were common in the breast sections. Hence, on superficial observation the sections of these cancer nodules of the liver appeared at first sight as a nucleated connective-tissue stroma, in which numerous large, round, oval, or elliptical mother-cells, containing numerous nuclei, were imbedded. On carefully focussing, however, it was easy to perceive that these apparently separate masses of protoplasm formed in fact portions of a complete network of nucleated cylinders, which had been divided by the section. As in the case of the mammary tumour, the cylinders appeared to consist of a mass of granular protoplasm with numerous nuclei imbedded, actual cell walls being nowhere distinguishable.

Nowhere did it appear that the hepatic cells had contributed by their multiplication to the formation of the cancer nodules; on the contrary, the actual structure seemed quite clearly to contradict any such inference. For some little distance around each little cancer nodule the cut end of single nucleated cylinders were frequently observed imbedded in the very substance of the hepatic acini. They always occupied, however, the vascular spaces or the meshes of the network of hepatic cells, and these appeared quite normal except in the immediate vicinity of the cancer nodules, where the chains of hepatic cells were often flattened somewhat, as though by the pressure of the growth. At the margins of the growths, in many places, nearly all the meshes formed by the compressed hepatic cell-chains were occupied by the nucleated cell cylinders of the cancer, the atrophied secreting tissue of the liver here taking a position corresponding to that of the connective-tissue stroma of the more central portion of the cancer nodules. The cancer nodules then would appear to have grown by a continuous extension of their nucleated cylinders into the adjacent hepatic parenchyma, the secreting cells of which seem to perish by atrophy without contributing to the substance of the growth. (See Microscopical Section, Nos. 2389 to 2393.)

No. 959, Medical Section. The greatly hypertrophied spleen. The spleen was nine and a half inches long, five and a half broad, by three thick in the middle. It weighed when fresh seventy ounces. Its margins presented several deep fissures (lobulated spleen). Beneath the peritoneal coat were a number of small cancer nodules. Sections of these cancer nodules showed their structure to be quite similar to that of the cancer nodules of the liver, except that the meshes of the nucleated cylinders were more irregular and the cylinders themselves were in many parts somewhat thicker. The relations of the margins of these nodules to the hypertrophied splenic tissue, so far as it could be made out, was essentially similar to what was observed in the liver. (See Microscopical Section, Nos. 2642 to 2655.)

No. 960, Medical Section. The uterus and its appendages. Each ovary is transformed into an oval somewhat nodulated scirrhous mass, an inch and three-quarters in long diameter. In sections no trace of Graafian follicles or ovules could be discerned. The scirrhous mass everywhere consisted of a tissue very similar to that above described in the breast. Simply the meshes of the nucleated cylinders were longer, so that when the section passed parallel to them they could sometimes be traced running parallel to each other for some distance; moreover, the connective-tissue stroma was more conspicuously fibrillated than in any of the other growths. (See Microscopical Section, Nos. 2449 to 2467.)

No. 956, Medical Section. The heart. No. 957, Medical Section. A portion of the pericardium. On the surface of the heart and of the parietal pericardium were a number of flat white thickenings, which were at first supposed to be carcinomatous. They proved on section to be composed of an imperfectly-fibrillated matrix, in which numerous small corpuscles were imbedded, and probably are to be referred to some previous inflammatory process, and not to the carcinomatous disease. (See Microscopical Section, Nos. 4690 to 4693.)

On reviewing this case we cannot fail to be struck first with the great similarity of the minute structure of all the morbid growths, although seated in quite diverse organs. Everywhere the normal tissue characteristic of the part affected appeared to be replaced by the morbid product, which was everywhere similar in texture.

This similarity of texture has long since been frequently observed in multiple carcinomatous growths and is the anatomical basis of the doctrine of a cancerous dyscrasis regarded as the constitutional cause of the local affections. It will readily be understood that if we could believe in a special modification of the blood giving rise to peculiar organizable exudations, we should expect the tissues thence derived to be everywhere characterized by some common structural features, the consequence of their common origin. Such a view as that of Koester, however, would equally well explain the circumstance referred to. For if the nucleated cylinders are, everywhere, transformed lymphatics, they must be expected to have very many features in common, combined with some minor differences in the calibre of the cylinders, the size of their meshes, &c., consequent upon the local variations in the character of the finer lymphatic nets.

Koester describes the network of cylinders in epithelial cancer as "cell cylinders" and was able by silver imbition to map out cell walls and to trace the continuity of the cancer cylinders with normal lymphatics lined by their characteristic epithelium. At the time the foregoing case was investigated his monograph had not yet reached me; since reading it I have caused sections of several other carcinomatous tumours to be treated with silver, by Dr. E. M. Schaeffer, one of the assistants at the Museum. In several instances results were obtained approximating those described by Koester, but they have not yet been sufficiently complete to justify me in forming a definite opinion. I hope to make this matter the subject of a future report. In the meantime, however, I cannot but admit the close correspondence between the outlines of the plexus of nucleated cylinders and the network obtained when the lymphatic capillaries are injected. And I may add that the general features of the morbid growths, above described, *viz.* a network of nucleated cylinders, interlacing with a connective-tissue stroma, are to be observed in quite a number of the sections of carcinomatous growths preserved in the Museum. In none of these cases, however, have I as yet been able to satisfy myself with Koester of the existence of a lumen in the central part of the nucleated cylinders, nor am I convinced that the normal lymphatic capillaries, lined by an epithelium, are alone capable of being transformed into the cancer cylinders. Much more probable does it appear to me that all the lymphatic spaces of the connective tissue are susceptible of this transformation, which would at once account for the voluminous character of the pathological network.

The presence or absence of cell walls in the protoplasm, through which the nuclei of the cancer cylinders are distributed, does not appear to me an essential point. Marked cell walls can be observed in the cylinders of several epithelial cancers in the Museum collection. On the other hand, Koester himself mentions that at times he found cylinders in which the nuclei were imbedded in a granular protoplasm in which no cell walls could be observed.

In the present state of our knowledge we may perhaps regard the cell wall as an indication of a comparatively advanced stage in the history of the individual cell, which at first consists merely of a nucleus surrounded by a mass of protoplasm. There seems then no difficulty in agreeing with Koester to regard such nucleated cylinders as I have described above as composed of cells too young to be possessed of walls, and placed in such close juxtaposition that no line of demarcation can be observed between the protoplasm of the several elements.

As to the mode in which the cells of these cancer cylinders arise, however, I find greater difficulties. I am not satisfied either by my own examinations or by the study of Koester's paper that they can be justly considered the progeny of the lymphatic endothelium. A view which should regard them as transformed white corpuscles, accumulated in the lymphatic passages, appears to me, if anything, a more probable interpretation. This would harmonize with the general absence of any demonstrable lumen, as well of demonstrable cell walls for the separate elements, at least in the earlier period of the history of individual cylinders; it would also harmonize with the close agreement generally observed between the elements of the youngest cylinders, *i. e.* those in the most peripheral parts of the growth, and ordinary granulation tissue.

I am not willing, however, to commit myself very warmly at present to the advocacy of any detailed views as to the mode of origin of the cancer cylinders. I desire simply to call attention to the probability, now almost a certainty, that they are conditioned in their form and direction by the pre-existing lymphatic passages, and am disposed to postpone any opinion as to their genesis till further facts have been accumulated.

In the case which has been described the primary growth was not received by me for investigation. I have therefore added a short account of a primary tumour of the breast, in which the structure was very similar to that observed in the secondary cancer of the breast in the case above described.

No. 5905, Surgical Section, is a scirrhous tumour of the female breast removed by a surgical operation in the spring of 1870. No particulars are recorded, and the subsequent history of the patient has not been reported.

A section through the piece shows that the mammary gland is represented by a small somewhat pyramidal mass, the apex of which terminates in the retracted nipple. The borders of the mass extend irregularly into the surrounding adipose tissue which constitutes the bulk of the specimen.

The case represents that very common variety of scirrhous in which the mammary gland is transformed into a small irregular cancerous mass, while the surrounding adipose tissue remains normal in bulk or even may actually increase in quantity. Nos. 3489, 3490, and Nos. 4616 to 4623, Microscopical Series, are sections of this tumour.

In these sections, as in the secondary mammary tumour in the former case, more or less modified gland lobules were but rarely encountered, and the greater part of the carcinomatous tissue appeared to consist simply of a network of nucleated cylinders imbedded in a connective-tissue stroma. In this case, however, the cancerous mass was imbedded in a more bulky mass of adipose tissue, the peripheral parts of which were quite normal. The separate fat-cells were much larger than in the first case, being from ·002 to ·004 inch in diameter, while in the first case the limits were ·001 to ·002 inch. This would appear to depend simply upon the degree of emaciation which existed in the first case at the time of death. At all events the anatomy of that part of the adipose tissue which adjoined the morbid growth was essentially the same in both cases. Extension of the cancer cylinders into the connective-tissue septæ between the fat-lobules, and the development of a more or less abundant nucleated matrix between the individual fat-cells of those lobules nearest the scirrhous tissue, were characteristic conditions in both cases.

The first of the plates which illustrate this paper represents a portion of preparation, No. 3489, Microscopical Section, which is taken from this second case, magnified 400 diameters. It exhibits a side view of a part of the network of nucleated cylinders. The granular character of the protoplasm, the imbedded nuclei and the manner in which the nucleated cylinders interlace with the connective-tissue stroma, are faithfully represented. The nuclei of the connective-tissue stroma are not so obvious, but few of them lying in the optical plane selected for representation.

The second plate represents a portion of one of the liver nodules in the first case. It is copied from preparation No. 2393, Microscopical Section, magnified 400 diameters. The section divides several of the nucleated cylinders in a direction nearly perpendicular to their course, and they appear hence, as above described, not unlike so many multinucleated or mother-cells.

Both photographs were made from carmine-stained preparations, mounted in balsam, and in both, therefore, the nucleated cylinders appear shrunk away from the connective-tissue stroma in which they are imbedded.

In conclusion, I must refer briefly to another interpretation of the nature and origin of such nucleated or cell cylinders as I have above described, which has been received with great favour for some few years.

Thiersch in his work on epithelial cancer explains the cell cylinders in that form of carcinoma, especially of the skin, as outgrowths from the lower layer of the epidermis and from the epithelium of the glandular apparatus.* Such an interpretation implies, of course, the possibility of demonstrating that the cell cylinders are continuous with the epithelial structures from which they are supposed to have budded forth. Such a continuity Thiersch believes himself to have observed in many of his sections. He admits, however, that it is often impossible to make it out, and explains this by supposing the connection to have originally existed but subsequently to have disappeared, pointing, in justification of the hypothesis, to the history of the normal development of the

* 'Der Epithelial-Krebs.' Von Dr. Carl Thiersch. Leipzig, 1865. Page 58.

embryo as affording many examples of epithelial formations, which proceed originally from the epithelium of the surface, but become disconnected with it at a later period of their growth. Thiersch did not fail to observe the fact of the frequent anastomoses of the cell cylinders, which he has very well figured in fig. 1, Taf. IX, but this circumstance did not shake his confidence in the view he had propounded.

Billroth maintains similar views to those of Thiersch, not merely for cancer of the skin, but also for mammary cancers, and in fact for cancer generally.* In the last edition of his lectures he mentions the view of Koester as "enticing," but thinks that "all his evidence in favour of this view is not tenable."†

I would call attention, however, to the fact that the manner in which the cell cylinders anastomose in at least many mammary cancers, points rather to the lymphatics than to the mammary-gland tissue. Moreover, when a mammary cancer returns after extirpation, or when secondary growths develop in distant organs, as in the case reported in this paper, the new formations are generally constructed upon the same structural plan as the original growth. To reconcile these facts with Billroth's views, we must resort to far more complicated and improbable hypotheses than those of Koester.

Billroth was well acquainted with the anastomosing cell cylinders in certain forms of mammary cancer. He describes such growths as the "tubular form." According to him, mammary cancer in general almost always begins "with a coincident enlargement of the small, round, epithelial cells in the acini, and with small celled infiltration of the connective tissue around them." He admits that it is "difficult to make out the further fate of the glandular acini," yet he has no doubt that in the tubular form "the acini do not maintain their form, but grow into the connective tissue as very thin cell cylinders, while it becomes infiltrated with cells." I mention with interest that fig. 144, page 643 of his work (*loc. cit.*), agrees precisely, so far as a woodcut can represent the microscopical appearances of a morbid growth, with portions of some of the sections of the two cancerous breasts described in this paper. In it, as in them, certain figures which undoubtedly represent more or less modified gland acini and ducts, lie side by side with the cell cylinders, which are variously shaped in accordance with their relation to the plane of the section. Both are imbedded in the same connective-tissue stroma, but in his figure, as in the Museum preparations, no anatomical connection between the two is shown. They are separated always by connective tissue.

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* 'General Surgical Pathology and Therapeutics.' By Theodore Billroth. Translated by Charles E. Hackley, M.D. New York, 1871.

† *Loc. cit.*, p. 631.

Nevertheless I am not prepared to deny that transformations of the true gland tissue of the parts involved, play a certain rôle in producing the texture of cancerous growths. But just how far they are atrophied and perish, how far at times a formative activity may lead to their transformation, are matters as to which it is difficult at present to form a judgment, and to which I hope to return at some future time.

Meanwhile I cannot avoid calling attention to two more of Billroth's woodcuts—fig. 148, page 648, and fig. 150, page 651 (*loc. cit.*)—the first representing the "extension of cancerous tumour into the fatty tissue about a lymphatic gland," the second "cellular infiltration of the fatty tissue in the periphery of a hard cancer of the breast." Either figure might have been drawn from the altered fat in the sections of the two breasts described in this paper, and the appearances are such that, as Billroth well remarks, "we can scarcely avoid thinking that in these cases also, white blood-cells escaping from the vessels cause the cellular infiltration." With this suggestion I heartily agree; but the real and difficult point in the analysis of cancerous tumours, as of so many pathological transformations, is to define with precision the exact part which the emigration of white blood-cells plays in the process, how far it is the principal, how far merely the accessory alteration. This report is offered as a preliminary contribution to the study of this question, now become so important in reforming our pathological doctrines.

I may mention, in conclusion, that the microscopical sections referred to were all made by Dr. E. M. Schaeffer, and that they are permanently preserved in the Microscopical Section of the Museum, where they may be examined by anyone interested in the subject.—*Report to the Surgeon-General of the United States' Army.*

ARMY MEDICAL MUSEUM, MICROSCOPICAL MUSEUM,
April 29th, 1872.

VII.—*The Theories of Cell Development.* By I. N. DANFORTH, M.D., Pathologist to St. Luke's Hospital, Chicago, U.S.A.

FROM a very early period in the history of medicine the idea prevailed that there must be some anatomical unit, some elementary part or particle, which, whatever might be its form or size, must be regarded as the true seat and home of vitality. Without the aid of optical instruments, or even of the dissector's scalpel, and at an age of the world when philosophy was but a tangled maze of fancies; when men, with curious persistency, shunned the plainest truths and blindly groped in darkness, even then the conviction seems to have forced itself upon many that, underlying the various tissues and organs of the body, there must be some vital element common to them all, and from which they were all originally differentiated. And this idea was not so much the product and result of close and vigorous analytical *thought* as it was the outgrowth of an inevitable necessity, namely, the necessity for a *somewhere* and a *something* which should serve as a biological starting-point. Hence we find Fallopius (1523–1562), Borellus (1656), Haller (1757), and others, constructing their rude, and to us preposterous, theories in answer to demands which were irresistible. But these early theories have for us little value or interest, save as antiquaries, since none of them clearly recognize the cell as the ultimate anatomical element. To the histologist of the present day it seems strange, and indeed almost incredible, that so many years should elapse between the real discovery of the cell and the comprehension of the one great fact that this little body is the starting-point of all development. There seems to be little doubt that Borellus actually saw the cell as long ago as 1656, since, as Dr. Tyson remarks, "he describes pus-corpuscles as animalcules, and even says he saw them delivering their eggs."* In truth, it is more than probable that what Borellus believed to be the act of "delivering eggs" was really the process of cell development by "budding" or "gemmation," so that this very important discovery was made two centuries before it was announced.

The first attempt at the construction of a systematic theory concerning the cell and its origin, seems to have been that of Wolf, about the year 1759. From this time, therefore, the cell may be said to have a connected history; although it is quite common to regard cell history as commencing many years later, with Schleiden and Schwann. Although the name of Wolf was well-nigh forgotten, and would have so remained but for the magic touch of the brilliant but erratic Huxley,† his doctrines have exercised no little influence upon the thoughts and writings of many of his followers. We shall presently see that Wolf is the *real*, while Schleiden is only the *putative*, father of that wayward child, free or spontaneous cell development.

If we carefully analyze the various cell theories which have appeared from the time of Wolf until the present—and which for convenience we may style "modern" theories—we shall find, first, that they are all hinged upon different conceptions concerning cell development; and secondly, that these different theories may be reduced to four classes, namely:—

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* 'The Cell Doctrine,' p. 16.

† 'British and Foreign Medico-Chirurgical Review,' October, 1853.

(*a.*) Theories which recognize free cell development.

(*b.*) Molecular or globular theories.

(*c.*) Theories which locate developmental power outside the nucleus.

(*d.*) Theories which confine developmental power to the nucleus alone.

No sooner was the cell recognized as the true and universal agent of development, than the importance of understanding its mode of development was seen; hence we at once encounter theories of cell development, and for many years these followed one another in rapid succession.

(*a.*) *Free Cell Development.*—This doctrine has at least one positive merit, namely, boldness. Commencing, as I have already intimated, with Wolf in 1759, it has encountered various *ups* and *downs*, until, at the present time, it is really the keystone of the doctrine of "spontaneous generation," so vigorously defended by Dr. H. Charlton Bastian. According to Wolf, "every organ is composed at first of a mass of clear, viscous, nutritive fluid, which possesses no organization of any kind, but is at most composed of globules."* Subsequently cells are developed, but they are "mere cavities, and not independent entities; organization is not affected by them, but they are the visible results of the action of the organizing power inherent in the living mass, or what Wolf calls the *vis essentialis*" (Huxley). This simply amounts to saying that the cell is merely a passive, not an active body; that it is only matter thrown into a convenient form for building material, as clay is thrown into the form of bricks for the same purpose, while the real builder is the so-called "*vis essentialis*" resident in the "clear, viscous, nutritive fluid." Twenty years ago it was necessary to combat this doctrine, but at the present day it does not even merit the compliment of criticism.

We encounter the next phase of free cell development in connection with the doctrines of Schleiden. This observer—the first to comprehend the fact that the cell is the true and ever-present basis of developmental action in vegetable tissues—acknowledged two distinct methods of cell growth; one of which, the so-called "*exogenous free cell formation,*" must be regarded as a true creative act, while the other ("*endogenous cell formation*") is a mere continuance of the process, or cell multiplication. In the midst of a perfectly structureless, clear, transparent fluid (*blastema, cytoblas-*

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* Huxley, *loc. cit.*

tema), granules are first seen, "some of which increase in size and assume the form of a minute vesicle, the nucleus of the future cell. On the addition of water to this, granules become apparent in its interior, and one of these, larger than the rest, appears to be the nucleolus. *Around* the nucleus, as is generally stated, the cell membrane is developed, and thus the cell is completed." * Virchow's account of Schleiden's theory does not exactly agree *in terms* with the one quoted above, although the practical difference amounts to nothing; it is as follows: † — "According to the view which was put forward in the first instance by Schleiden, and accepted by Schwann, the connection between the three coexistent cell constituents was long thought to be on this wise, — that the nucleolus was the first to show itself in the development of tissues by separating out of a formative fluid (*blastema*, *cytoblastema*), that it quickly attained a certain size, that *then* fine granules were precipitated out of the blastema and settled around it, and that about these then condensed a membrane. That in this way a nucleus was completed, about which new matter gradually gathered, and in due time produced a little membrane (the celebrated 'watch-glass' form)." In the one case, granules are first produced, and, by their aggregation, the nuclei are afterwards formed; in the other, the nuclei take the precedence, the granules being a secondary product. Seeing that the nucleus was discovered several years previously (1833) by Brown, of Edinburgh, and that Schleiden had conferred upon it the name of "cytoblast" or "cell-bud," thus directly implying his belief in its importance in relation to cell growth, it is more than probable that Virchow's account is the more correct one. Schwann, whose observations seem to have been confined chiefly to animal structures, adopted the views of Schleiden concerning the development of vegetable cells, and applied them to the genesis of the cells of animals. He "was of opinion that the extracellular formation of cells—that is, their development in free blastema—was the most frequent mode of their production in animals." ‡ At this period, therefore, the theory of free or spontaneous cell development was generally accepted by histologists; and, seeing that Schleiden and Schwann gave to this notion the support of their great authority, it is by no means a matter of surprise that it found general favour.

Passing by the more recent advocates of spontaneous cell-growth, especially Bennett, of Edinburgh, Carpenter, and Todd and Bowman,§ with the remark that they are simply recasts of the views of Schleiden, we come to that phase of spontaneity which, if established, fully demonstrates the possibility and indeed the *constancy*, of the spontaneous origin, not of individual cells alone, but of living beings.

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* Peaslee's 'Human Histology,' p. 120.

† 'Cellular Pathology,' p. 35.

‡ Stricker's 'Histology,' vol. i., p. 34.

§ 'The Physiological Anatomy and Physiology of Man,' edition of 1857. It is proper to observe that in the last edition (1866), now issuing in parts under the editorship of Dr. Lionel Beale, the well-known cell theory of the latter is presented.

It does not come within the limits or scope of the present article to review the history of the long, and as yet unsettled, contest concerning the spontaneous evolution of cells, or of those simple aggregations of cells which form the lowest types of living things. For a very complete review of this subject, I take great pleasure in referring to the essay by Professor J. C. Dalton, in the 'New York Medical Journal' for February, 1872. The doctrine of spontaneous generation was generally believed in by the ancients; was disproved, in all except certain "exceptional" cases, by Regnier de Graaf (1672) and Von Baer (1827); was revived by Needham (1748), and again controverted by Spallanzani (1775); was vigorously assailed by Schultze and Schwann (1836 and 1837), and for twenty years after the period last mentioned was generally abandoned by scientists as untenable. "The production of living beings without parents was a theory admitted to have no reasonable basis for its support, and was regarded simply as a curious relic of antiquity."*

But in the year 1858 the doctrine of spontaneous evolution was revived by Pouchet, of France, as a sort of buttress to support the conclusions to which he was forced by the revelations of modern geology. Pasteur, however, and several other members of the French Academy, repeated Pouchet's experiments with entirely negative results, and again this troublesome ghost seemed upon the point of being laid, when Professor H. Charlton Bastian published his series of experiments, and announced himself as "content" and "justified" in "believing that living things may and do arise *de novo*." According to Bastian, various unicellular organisms are pretty constantly developed in boiled and hermetically-sealed infusions of hay, turnip, &c., and also in certain saline solutions treated in like manner. The reader is referred to Dr. Bastian's work, the 'Beginnings of Life,' recently published, for an extended account of his very interesting experiments, and the accompanying illustrative figures. He claims to have demonstrated that monads, bacteria, torulæ, vibrios, leptothrix filaments, fungus spores, and various unclassified organisms of cellular structure, are developed from solutions of organic and saline substances absolutely destitute of living germs; hence they must originate *de novo*. This, of course, is the very essence of free cell development, or rather it is something more than that, since it confers upon any solution of organic or saline matters containing the chemical elements of protoplasm, the power not only to generate cells, but to spur them forward to that subsequent differentiation which results in the (not *lawful*, but) lawless creation of living beings. It carries us back to the days of Wolf, and substitutes for specific germs, endowed with vital force for a specific and unchangeable purpose, a something (which we may as well call a "*vis essentialis*" as anything else) by virtue of which dead matter is brought to life, boiled hay is made a creator, and solutions of salts forget the laws of crystallization and take to themselves the power of the genesis of new and unheard-of *living* (!) beings. For my own part, I cannot accept the doctrine of spontaneous cell formation in any form or to any extent, or the doctrine of heterogenesis (which is the same thing in different dress), until, like Pouchet, I shall be blessed with that boundless faith which sees in spontaneity the very beginning of animated existence, and in the higher order of beings, even up to man himself, only the result of "the ascending development of organized beings upon the globe."

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* Dalton, *loc. cit.*

(*b.*) *Globular or Molecular Theories.*—The globule theory, according to Virchow, was partly the outgrowth of the reaction against the fibre theory of Haller, partly the result of optical illusions from the unskilful use of microscopes which, at the very best, were infinitely inferior to those of the present day. It is difficult to understand the precise relation of the globule or molecule theory to the cell in the minds of the earlier writers, for the following reasons: First, several observers, following Leuwenhoek, believed that the tissues were developed directly from globules, without the intervention of cells; secondly, the terms "globule," "molecule," and "granule" were, and indeed still *are*, so interchangeably used, especially among the Germans, that neither term seems to have, or to have *had*, any very definite meaning; and thirdly, it is, to say the least, very probable that cells were frequently described as globules, and globules were sometimes described as cells. Although the terms "globule," "granule," and "molecule" are now understood to refer to bodies which are morphologically, and possibly chemically, distinct, they usually seem to have indicated to the earlier, as well as to some recent, writers, bodies smaller than cells, but having to cells a developmental relation.

The writings of Milne-Edwards gave general currency to the globule theory; but he does not seem to have deemed it necessary that globules should be converted into cells before proceeding to form tissues; but, as pointed out by Tyson, "there is little doubt but that many of these so-called globules described by Edwards were really cells, seen with indifferent instruments, and further distorted by the glare of direct sunlight."*

On the other hand, Arnold and Baumgärtner recognized the cell as an intermediate stage of development. "A cell was considered to be produced by the globules arranging themselves in a spherical form, so as to constitute a membrane, within which other globules remained, and formed contents."* But they also believed that fibres were formed of elementary granules drawn up in a line.

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* 'The Cell Doctrine,' p. 24.

Concerning the so-called "investment" or "cluster" theory, which for a time was pretty generally accepted, Virchow says "the upholders of this theory imagined that originally a number of elementary globules existed scattered through a fluid; but that, under certain circumstances, they gathered together, not in the form of vesicular membranes, but so as to constitute a compact heap (*mass, cluster,—Klümpchen*), and that this globe was the starting-point of all further development,—a membrane being formed outside and a nucleus inside by the differentiation of the mass by apposition or intussusception."† This theory is a most singular conglomeration of truth and error: of truth, in that it concedes what we now know so well, namely, that the membrane is not a necessary part of the *living* cell; of error, in that it practically recognizes free cell development.

Probably the most strenuous and persistent advocate of the globular (or, to adopt his own terminology, the molecular) theory at the present time is Professor Bennett, the eminent pathologist of Edinburgh. His theory is best given in his own words:—"The ultimate parts of organization are not cells nor nuclei, but the minute molecules from which these are formed. They possess independent physical and vital properties, which enable them to unite and arrange themselves so as to produce higher forms. Among these are nuclei, cells, fibres, and membranes, all of which may be produced directly from molecules. The development and growth of organic tissues is owing to the successive formation of histogenetic and histolytic molecules. The breaking down of one substance is often the necessary step to the formation of another; so that the histolytic or degenerative molecules of one period become the histogenetic or formative molecules of another."‡ On the succeeding page he adds, "As to development, the molecular is the basis of all tissues. The first step in the process of organic formation is the production of organic fluid; the second, the precipitation in it of organic molecules, from which, according to the molecular law of growth, all other textures are derived either directly or indirectly,"—that is, with or without the intervention of cells.

It will readily be seen that the theory of free cell development, as originally stated by Wolf and modified by Schleiden, and the globular or molecular theory, are, practically, one and the same thing,—the difference being in *terms*, not in results. Both *mean* spontaneous cell formation. Both alike deny that a cell is necessary to the development of a new cell; but, while the former comes squarely to the point and *says* what it means, the latter makes use of the molecule as a sort of sugar-coat for that unpalatable dose, spontaneity.—*The Lens*, July.

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* Virchow, 'Cellular Pathology,' p. 53.

† *Op. cit.*, p. 53.

‡ Bennett's Lectures, p. 118.

VIII.—*Draw-tubes* versus *Deep Eye-pieces.*

By M. A. Prazmowski.

The magnifying power of any microscope depends upon the three following conditions:—

1st. The focal distance of the objective (equal to the principal focus of a small equivalent lens placed in the optical centre of objective combination).

2nd. The magnifying power of the eye-piece.

3rd. The distance between the optical centre of the objective combination and the eye-piece.

By varying the last two conditions, for instance, a long tube and low eye-piece, or a short tube and a high eye-piece, we may obtain results identical, so far as magnifying power is concerned. In other words, designating by L and l the longer and the shorter tubes, and by O and o the stronger and feebler eye-pieces, $\frac{L}{O} = \frac{l}{o}$.

For the sake of convenience, we leave out of this statement the slight difference in the power of the objective produced by the movements of the collar for the purpose of obtaining the best definition with the two eye-pieces.

It is a practical fact that, with a given magnifying power, the image obtained by a short tube and high eye-piece is more perfect, clearer, and brighter than that obtained by the use of a long tube and low eye-piece.

Any observer may verify this fact by employing successively the two methods for the purpose of augmenting, or perhaps we may say exaggerating, the power of a given objective beyond its best working limits. It will be found that the richness of detail is better preserved with the short tube and high ocular than with the long tube and low ocular.

By a careful study of the conditions present in the relations of the eye-piece and objective, I think I am able to show that the different results above noted do not depend upon any defect in the correction of the spherical or chromatic aberrations of the objective combination, but are produced by the variable amount of interference of the rays of light in the two given conditions, the fringes of interference becoming larger and more injurious to the image as the tube is lengthened and the luminous cone becomes more acute.

It is well known that the capacity of an objective to display the details of a surface depends largely upon its angle of aperture; the larger this angle, the finer the objective. We make the single remark, that the magnitude of the angle of aperture has reference only to those rays that enter into the formation of the image. If the aberrations are not corrected through the whole extent of the angle, those rays that find entrance through the uncorrected portions, unless for the formation of the image only, serve to flood the field with light, in which the image is lost, drowned. The larger the angle of aperture, the larger the surface of the back lens of the objective. The circle formed by this back lens, which receives all the light introduced into the objective, is the base of a cone, of which the summit is found in the conjugate focus of the objective, at the point where is formed the image seen by the eye-piece. In objectives of high power the base of this cone, as compared with the distance separating it from the eye-piece, is very small; the cone is very pointed, and the rays intersecting one another at its summit make an angle necessarily very acute, and more acute in proportion as the summit is removed from the objective.

We know that rays of light crossing one another at very acute angles do give rise to this phenomenon of *interference*, and that the fringes are larger in proportion as the angle of intersection is diminished. Applying this principle to the problem under consideration, we find that the rays passing near the centre of the objective must give rise to this phenomenon, and that as the rays pass from the centre the fringes of interference will be less, and the definition consequently finer and sharper. This improvement in the definition will continue until the rays approach the border of the lens, when, from imperfect corrections of *aberrations*, or too great an angle of incidence, they no longer enter into the formation of the image.

We may say, for the sake of simplicity, that the extreme rays nearest the borders of the objective form a cone, the base of which is upon the objective and the apex in the focus of the eye-piece. In proportion as we increase the length of the tube of the microscope, or, what is the same thing, the distance between the objective and eye-piece, the luminous cone becomes more pointed, for the base of it is unaltered, while the height of it is increased. The angle of intersection of the rays at the summit is diminished, while at the same time the fringes of interference rapidly increase in width, and, mingled with details of the image of the object, they only serve to mask it. The image produced at a less distance from the objective is less affected by these fringes when magnified by a higher ocular, and will be found, although enlarged to the same extent, to be more sharply or clearly defined, and more exact in all its minute representations of real structure.—*The Lens,* July.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Structure of the Rods of Corti.—Dr. Urban Pritchard has communicated a short but important paper on this subject to the Royal Society, May 30th, from which we may give the following account of these curious rods:—In a general view of the rods from above, they appear similar to two rows of pianoforte hammers, rather than like the keys of that instrument, to which they have been likened. In a lateral view, these two rows of rods are seen sloping towards each other, like the rafters of a gabled roof. The rods consist of a shaft and two enlarged extremities, but the two rows differ considerably in form; the inner rods are attached by their lower extremities to the membrana basilaris at its junction with the lower lip of the limbus, and just external to the spot where the nerve filaments emerge; they are directed outwards and upwards, with a slight undulation to meet the outer rods. The lower extremity is enlarged and rounded, gradually tapering to the shaft, which is cylindrical; the upper extremity is somewhat cuboid in form, but the outer surface is deeply concave, and the upper lip of the concavity is prolonged into a process. The outer rods are attached to the membrana basilaris by a broad base, which also gradually tapers to a cylindrical shaft. Their upper extremity is less cuboid in form, and presents a convex internal surface, which articulates with the corresponding concavity in the inner rods just mentioned; from the outer and upper part there extends outwards a slender process. One of the most important features with regard to these rods is their relative length. Most authors state that there is very little difference in the length of the two rods; in this, however, they are much mistaken; for not only do the two sets of rods differ in this respect, but the length of each varies according to its position on the cochlea. Thus, at the base, the outer rods are as nearly as possible equal in length to the inner; but proceeding upwards, both rows increase in length with great regularity, although not in the same ratio, the outer increasing with much greater rapidity, so that near the apex they are twice the length of the inner. It was generally supposed, *a priori*, that these rods were graduated so as to distinguish the most minute variation of tone, but no one until now has been able to demonstrate this. The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{200}$ of an inch. The number of rods in each row is not the same, there being about three of the inner to two of the outer; and, according to calculation, there are about 5200 inner rods and 3500 outer in the whole cochlea. Most authors, with the exception of Deiters, describe nuclei situated in various parts of these rods, principally in the lower extremities; but although seen from above this appears to be the case, on closer observation these so-called nuclei of the rods are found to be nothing more than the nuclei of cells surrounding them.

The Early Stages of the American Lobster (*Homarus Americanus*).—These are the subject of an able paper of some length in the June number of the 'American Journal of Science,' to which we refer our readers for more details. The stages are all those after the lobster has left the egg, as those within the ovum are well known already. The author, Mr. S. J. Smith, makes four stages in the post-ovum development of this animal. These he then minutely describes and figures, and he considers that the fourth is the adult stage. He thinks, too, that there is probably another stage between the third and fourth.

A Microscopical Examination of the Skin in Impetigo contagiosa.—An interesting paper on this subject appears from the pen of Dr. H. G. Piffard in the 'New York Medical Journal,' June, 1872. The author says that the first examination was made March 7th. By this time the potash had effected an almost complete solution of the crusts, with the exception of a few small, whitish masses, which remained undissolved. One of these was transferred to a glass slide, a little glycerine added, and a thin covering glass applied. Upon examination with a $\frac{1}{5}$ (Grunow) little was found to attract attention except a few collections of very transparent epidermis, without visible nuclei, many oil-globules, and some granular matter. The first half-hour yielded, with the exception of the above, but negative results. At this time a slight modification of the method of illumination brought to view a minute circular body whose exceeding transparency had hitherto rendered it difficult of detection. Applying a higher power ($\frac{1}{10}$ Grunow), and carefully focussing, the body exhibited a peculiarity of form which was exceedingly striking. The effect produced by depressing the objective was similar to that with which we are all familiar in the case of the red blood corpuscle, namely, the border first came into focus, as evidenced by the bright outline and dark centre, and then, upon further depression, the border became dark and the centre brighter. Reversal of the movement produced a reversed effect. The first impression was, that the body under examination was in reality a blood corpuscle, so thoroughly was its appearance counterfeited; but its minute size, and the fact that it had lain two days in a potash solution, precluded this possibility. Further search detected six or seven similar bodies in the same field, and prolonged examination of the preparation, and of others upon succeeding days, enabled me to determine the few following facts:—The bodies, which were unquestionably vegetable organisms, were generally circular in outline, a few being oval. Their average diameter was $\frac{1}{5000}$ of an inch. Their form, as determined by focussing, *appeared* to be the same as that of the red blood corpuscle, namely, a biconcave disk. The spores were usually isolated, but occasionally two or three, and in one instance four, were found together. In this latter case, three of them were circular, while the terminal at one extremity of the chain was oval. In another case the two-days-old crust was placed in a three-per-cent. solution of potassa fusa, while a small fragment of the older crust was macerated for a few minutes in a mixture of liquor potassæ and glycerine, and immediately examined with a $\frac{1}{15}$-inch objective (Wales). The field presented was extremely striking. Not only were the hæmo-discoid bodies, which we had previously encountered, present in abundance, but others exhibiting an almost endless variety of form. This unexpected and puzzling difference of form naturally excited the liveliest curiosity, and led to the most careful examination, with a view to account for it if possible. It seemed improbable that the little hæmodiscoids, which at first we supposed to be the type-form, could have developed into many of the irregular forms presented. This view had to be abandoned, and search was made for some other simpler form or type which would permit of the variations noticed. Among others we observed a body resembling a spermatozoon, that is, with a round or rather oval head, and a thin filamentary tail, slowly *moving* across the field. We say moving in contradistinction to *moved* (by currents in the fluid). This motion was accompanied and probably caused by slow undulatory movements of the tail, nothing like the quick, jerking movement of spermatozoa, but resembling those observed in many other protophytic organisms. In another instance the head was elongated and in others again the tail consisted of two parallel filaments united below, and directly continuous above with the dilated cephalic extremity (so to speak). In others there were one or more slight dilatations along the course of the tail. Evidence of apparently vital movement was not lacking in connection with some other individual spores upon the slide. One of the most striking and unmistakable instances of this was witnessed in the case of a body composed of a chain of three rounded portions, to one extremity of which was attached a filamentary prolongation, itself terminated by a minute rounded body. The whole outline of this curiously-shaped body could be distinctly seen, with the exception of a small portion upon one side, which was partially obscured by some granular matter. At this time, attention was diverted from the microscope for a few minutes, not more than ten, and upon returning to the instrument, instead of finding the object last observed, in its place was one of a somewhat different form, recognized, however, as the same body by its relations to the granular matter above referred to. Observation was continued, in order to detect further changes, if any should occur. In a few minutes the extremity of the tail curved slowly up, and attached itself to the main body, forming a loop. This movement was distinctly visible. The slide was now temporarily put aside, and attention was given to the crust which had been digesting in the potash solution for (by this time) about two hours. The crust was found to be converted into a viscid gelatinous mass, due to the action of the potash upon the pus of which it was in part composed. Portions of this jelly were placed upon slides and immediately sealed with Bell's cement. Examination revealed new shapes, among others an appearance such as would be produced by stretching a thin india-rubber ring. These elongated bodies varied greatly in size. Their breadth was nearly uniform, being about the $\frac{1}{10000}$ of an inch, but in length they measured from the $\frac{1}{4000}$ to the $\frac{1}{750}$ of an inch, as determined by an eye-piece micrometer. Another body appeared to be composed of two concentric rings. How these were formed was at first not very clear, but subsequently became easily explainable. The essential portion of the observation was finally completed by the discovery of certain fine filaments, not more than the $\frac{1}{30000}$ of an inch in breadth, but whose length varied from the $\frac{1}{2000}$ to the $\frac{1}{300}$ of an inch. The presence of these filaments readily accounted for the development of the various forms hitherto noticed, from the simple hæmo-discoid to the more complicated varieties. No direct observation of the transformation of the filaments into other forms was witnessed, but it seems to me to be a fair inference that such change occurs. In addition to the well-marked bodies described, there were present epidermic cells, and a little oil, and besides many granular or rather molecular bodies, some slightly elongated, such as are found in most animal and vegetable infusions, and upon whose exact nature it would be somewhat hazardous to pronounce. These little bodies, as is usually the case, were in rapid motion. No connection could be traced between the epidermis and the spores; occasionally, to be sure, a spore would be found upon or beneath an epidermic cell, but the occurrence was probably accidental, and nothing like the intimate relationship which exists between the epidermis and the *microsporon furfur* was detectable. No hairs were visible in any of the preparations, and hence nothing can be said concerning them. Observation of the circular spores, especially the larger ones, with a $\frac{1}{16}$ immersion of Powell and Lealand upon their grand binocular stand, the property of my friend Prof. Arnold, leads me to suspect that their form is not that of a section of a cylinder with central depressions, but rather that they are rings. It is possible, however, that in the smaller ones the hole in the ring is occluded by subsequently-deposited material. The difficulty of deciding this point can be readily appreciated when the minute size of the organisms is borne in mind. The following conclusions seem fairly deducible from the observations above recorded: The bodies are unquestionably vegetable organisms. Their primitive form is probably filamentary. From the filament the others are developed. Their presence explains the contagiousness of the affection in which they are found, and brings it into the class of the Parasitic affections of the Skin.

Diseased Bone. Giant Bone-cells.—The 'British Medical Journal,' July 13th, gives an article on some important discoveries of Kölliker, relative to disease in bone. Robin has described under the name of *myéloplaxes*, and Virchow and other German histologists under the name of giant or colossal cells (*Riesenzellen*), certain multinucleated masses situated on the external part of the marrow of bone. These bodies have been subjected to careful research by Kölliker; the results of whose investigations, as communicated recently to the Würzburg Physico-Medical Society, point to an intimate connection between the presence of these bodies and the process of resorption of bone. Kölliker finds that, during resorption in healthy bones and teeth, the surface where this process is taking place presents fine lacunæ (commonly termed Howship's lacunæ); and that these lacunæ contain giant cells. Generally there is one cell in a lacuna, which it closely fits; sometimes a single cell fills two lacunæ; and in other cases there are several cells in a large lacuna. Considering these cells to be important agents in the removal of osseous or dental tissue, he designates them "osteoclasts" (or "osteophages"). Lacunæ containing osteoclasts have been found by Kölliker both in the interior and on the exterior of bones. Internally, they exist in ossifying cartilage, just beyond the bone, where ultimately cancelli are formed, as in the bodies of the vertebræ and the ends of the long bones. They also are present on the walls of the larger medullary cavities in process of development; as in the diploe of the cranial bones, in the medullary canals of long bones; and the development of the frontal, sphenoidal, and other sinuses of the head and face, is intimately connected with their presence in these parts. On the external parts of bones they exist in the following situations: the dental grooves of the embryonic jaws; many parts of the bones limiting the cranial cavity, both at the base and at the sides and summit; on the walls of the vertebral canal and on the arches and bodies of the vertebræ; on the walls of the orbit and of the nasal fossæ; on the coronoid and condyloid processes of the lower jaw, and on the zygomatic process of the upper jaw; in the situations of the foramina and canals by which many of the bones are perforated, such as the foramen ovale, the foramen opticum, &c.; and on certain parts of the bones of the trunk and extremities, especially in the neighbourhood of the articular ends of the bones; as well as on the surface of the temporary teeth, where they are abundantly developed while these are undergoing absorption. Having described the situation where these osteoclasts are found, Kölliker proceeds to comment on their signification. Hitherto, he says, the normal resorption of bone has been attributed either to the chemical action of the fluids or of the medulla, or to some not very well-defined minute process. Thus Virchow supposes the osseous substance to pass into simple marrow-cells; Bredichin and Rindfleisch regard the "myéloplaxes" or osteoclasts as bone-cells set free by removal of the earthy matter; and Billroth believes that, in cases of disease, the bone is removed by the pressure of granulation-cells. Kölliker's experiments have led him to the conclusion that the resorption of osseous and dental tissue does not consist in a mere transformation of its cellular and other elements, but in a removal of them; and that the multinucleated cells or osteoclasts are in no way formed from pre-existent osseous or dental tissue. The proofs in support of this view are the following:—1. A bone or tooth, at the parts where it is studded with osteoclasts, shows no sign of transformation of its cells, such as enlargement, multiplication of nuclei, &c.; and it is extremely common to find, on the walls of the lacunæ which contain the osteoclasts, partial erosions in the tissue, which in other respects presents no change. 2. The osteoclasts can always be readily separated from the osseous tissue; there is no intimate connection or gradual transition between them. 3. After the absorption of the bone, small splinters not unfrequently remain; these contain no cells, and cannot be transformed into cells. 4. The occurrence of osteoclasts on the surface of the ivory of the milk-teeth shows that no elements of the dental tissue that is undergoing absorption can enter into their formation. 5. A most striking fact is the presence of osteoclasts on the surface of ivory pegs that have been driven into bone. Billroth has found that these pegs become eroded; and Kölliker has found them to present on their surface not only lacunæ, but also osteoclasts. The osteoclasts, then, do not arise from pre-existent osseous or dental tissue; and Kölliker believes that they belong to the same category as the osteoblasts, which perform an important part in the formation of bones. His reasons are as follows:—First, transitional forms are observed. Secondly, most of the osseous surfaces, which at the time when absorption takes place are covered with osteoclasts, originally present osteoblasts; and the gradual intrusion of the former among the latter can be noticed by direct observation. Thirdly, osseous surfaces that have been undergoing absorption may again take on growth; and then there is a re-formation of osteoblastic cells, partly through the splitting up of the multinucleated osteoclasts. This is observed in the jaws. Before and at the commencement of the formation of the teeth, the lower jaw shows osteoblasts in the situation of the future dental groove. After this, when the dental sacs and teeth are being formed, the bony substance is removed, and a large furrow is formed in the jaw. Lastly, when the eruption of the teeth takes place, there is again a formation of bone at their roots, leading to the development of the alveoli. As to the manner in which the osteoclasts are concerned in the removal of bone, Kölliker has not arrived at a definite conclusion; but he considers the following points worthy of note:—1. The removal does not consist in an absorption of the earthy matter as in osteomalachia; it seems rather, that both the organic and the inorganic material are removed simultaneously. 2. The removal of the hard tissue is not preceded by crumbling, but takes place gradually, leaving the bone with well-defined and smooth edges and surfaces. 3. The surface of bones undergoing absorption, where osteoclasts are present, have not an acid reaction. 4. Two experiments, made in order to ascertain whether the osteoclasts yielded any slightly acid or alkaline material having a solvent action on the bones, were attended with negative results. 5. In many cases the osteoclasts present clusters of fine short hairs or filaments on the side in contact with the bone.

The Microscopy of the Blood in Syphilis.—We have already alluded to this subject. However, the 'Lancet' has taken the matter up also, and it gives (June 22) an excellent leading article upon it. Dr. Lostorfer, who, it seems, was specially involved in the matter, has supplied Stricker's 'Medizinische Jahrbücher' with the facts which have led him to the position he has now taken up on the affirmative side of the question. He was supplied, it appears, by Stricker, who appends a note to the paper, with a number of test specimens; and arrived, on the whole, at very correct conclusions. A drop of blood was taken from the skin, and as quickly as possible placed upon a clean object-glass, and covered. Twelve glasses, with drops belonging both to syphilitic and non-syphilitic patients, and properly numbered for subsequent identification, were then placed in a moist chamber, and were examined from day to day with a Hartnack's immersion No. 10, ocular No. 3. During the first day or two, vibrios, bacteria, and the early stages of development of sarcina, were commonly met with. On the third, or still more frequently on the fourth or fifth day, and occasionally even after twenty-four hours, small bright corpuscles made their appearance, which were partly at rest and partly in vibratory motion; some of them exhibited a small process. By the sixth day these corpuscles had increased in size and number. Many of these larger ones exhibited the above-mentioned process; and this then distinctly appeared as a bud. In a few it was so large that it was very little less than the parent cell. During the subsequent days the corpuscles continuously augmented in size; so that a few soon attained the magnitude of shrunken red corpuscles, or even exceeded them. At the same time smaller ones of all sizes were present. Gemmation was frequently perceived, many corpuscles presenting not one bud only but several, which were in some instances pedunculated, and in others directly applied to the parent cells. Cases also occurred where the secondary buds appeared upon the surface of the primary ones. Most of these gemmæ were of irregular form, but some were spherical. About the eighth or tenth day a vacuole formed within the larger corpuscles, which gradually increased in size till ultimately its walls appeared to consist only of a thin lamina, presenting a double contour under the microscope. At this period the corpuscles had attained their final development, and underwent no further changes even when the test could be preserved for a month, which of course was only of exceptional occurrence. Dr. Lostorfer also examined the characters of the corpuscles with different fluids, as with solution of sugar, distilled water, and Pasteur's fluid—namely, one-half per cent. of solution of common salt and one per cent. of solution of acetic acid. These fluids, notwithstanding their diversity, produced similar effects during the earlier period of the development of the corpuscles, causing them to shrivel, and preventing any further growth. Even at the sixth day some shrivelling occurred on the addition of these fluids and dilute solutions of sugar; but when distilled water was added at this period the vacuolæ enlarged rapidly. Gemmation took place in the form of long tubules, which closely resembled the germ tubes of fungi. The number of the corpuscles varied to a very great extent. In one instance he counted fifty of them in the field of the microscope at one time on the fourth day; but he is unable to say whether any proportion exists between their number and the severity of the symptoms of syphilis present. Dr. Lostorfer states that long before the discovery of these corpuscles he had carefully examined the blood of healthy persons without finding anything similar to them; and that during the last three months he has systematically subjected healthy blood specimens to the same treatment, as well as blood taken from those suffering from gonorrhœa, diphtheritic ulcers, lupus, and from typhus patients, without in any instance observing their development. He therefore thinks he is justified in terming them syphilis corpuscles. He is of opinion that temperature is a matter of great importance affecting their development; for when the temperature fell below a certain point, not yet exactly ascertained, the results were negative. He gives the appearances in thirteen cases, which show that the corpuscles are not associated with any particular form or stage of syphilis, but manifest themselves both before and after the occurrence of the eruptions and in tertiary affections. He guards himself from saying they in any way constitute the carriers of the infection; and, as regards their origin, it is uncertain whether they are introduced with the syphilitic poison, or whether the blood is so altered in syphilis as to permit their growth when derived from other sources. In the note appended by Stricker to Lostorfer's paper, he says that he tested Dr. Lostorfer's ability to diagnose syphilitic blood under the microscope on six occasions. On the first he sent him twelve proof specimens, of which Nos. 8 and 9 were taken from healthy persons, and the remainder from three different patients suffering from syphilis. In a few days Lostorfer replied:—"Nos. 8 and 9 non-syphilitic; two preparations spoilt; the remainder syphilitic." In the second list there were seven numbered proofs, and in the third nine proofs, and in each of these the syphilitic cases were correctly diagnosed. In the fourth list, however, no results were obtained; but this Lostorfer explained by stating that all the specimens had accidentally been exposed to a very low temperature (– 12° C.). The remaining tests, including one by Professor Hebra, seem to have been very fairly made out. Whatever may be the ultimate conclusion arrived at, enough has certainly been ascertained to lead to attention being paid to the subject; and as the field of observation is unfortunately only too common, it is probable that positive results will soon be obtained. High powers, immersion lenses, and some experience with the moist chamber, are indispensable for pursuing the inquiry.

Spontaneous Generation: Arguments against.—A very able and clearly-reasoned paper, though it is against the theory, is that of Mr. W. N. Hartley, F.C.S., of King's College Laboratory. It is to be found in full in the 'Proceedings of the Royal Society,' No. 132, 1872. The paper is of some length, and should be consulted by those interested in the matter. We merely give the author's account of the well-known Child and Wyman experiments. He says it has long been established by Pasteur, Payen, and other experimenters, that a temperature of less than 130° C. is insufficient to destroy all trace of life if the germs or spores are not immersed in a liquid; this is a fact admitted on all sides. In that case it is not difficult to understand how, in the experiments of Dr. Child and Prof. Wyman, organisms have been found in liquids to which air only which had passed through red-hot pipes was admitted. The former took a bulb with two narrow necks or tubes, and containing the experimental liquid; one tube was connected by a cork boiled in water with a red-hot porcelain tube filled with pumice, and connected with a gas-holder; the other tube dipped into sulphuric acid: the liquid was boiled for ten or fifteen minutes, and heated air was made to pass through the apparatus till the liquid and flask were cool. "When the bulb is quite cool, the necks are sealed by means of a lamp." In Wyman's experiments an apparatus of much the same kind was used; but the liquids in four cases were boiled for from five to ten minutes in a Papin's digester, under a pressure of from two to five atmospheres, or at a temperature of 120° to 150° C. Large flasks of 500 cub. centims., and even 850 cub. centims. capacity were used, containing 17 cub. centims. to 50 cub. centims., or thereabouts, of solution, so that in some cases only $\frac{1}{25}$ of their capacity was occupied; the air admitted was passed through red-hot iron pipes filled with iron wires. There is a similarity between these two sets of experiments; the flasks were not entirely in contact with the hot liquid. We see also, from the few words quoted from the description of Child's experiments, that a sufficient space intervened between the red-hot tube and the bulb to allow of the heated air becoming cool before it entered the glass bulb; it cannot be said, therefore, that the entering air was so hot as to destroy whatever living thing might be attached to the glass. The precautions taken, then, were not sufficient to render the experiments trustworthy, more particularly in the case of Wyman's work, because there he had an immensely large surface untouched by fluid, and naturally he obtained more results in favour of the view of evolution than any other experimenter. Bastian's own experiments are open to the same objection; in fact, it seems that much work has been rendered faulty by this neglect of bringing every part of the interior surface of the containing vessels in contact with the heated liquid. We have positive proof that such is the case when, as Bastian himself states, "it has long been known that a boiled fluid extremely prone to change will not yield infusoria if the vessel in which it is contained is filled with the fluid;" the commercial method of preparing cooked meat depends upon this to a great extent.

The Power of various Substances to destroy Protoplasmic and Fungus Life has been inquired into by Dr. F. Crace-Calvert, F.R.S., who has published a paper on the subject in the 'Proceedings of the Royal Society.' His experiments consisted in adding to a solution of albumen swarming with microscopic life, one-thousandth part of the following substances, and examining the result after one and sixteen days. The experiments were begun on the 20th Sept., 1871, the solutions being kept at a temperature of 15°—18° C. In the standard solution the amount of life and putrescence increased during the whole of the time. The first class includes those substances which completely destroyed the locomotive power of the vibrios immediately, and completely prevented their regaining it during the time the experiments were conducted:—Cresylic acid. The second class contains those compounds which nearly destroyed the locomotive power of all the vibrios present when added, and afterwards only one or two could be seen swimming about in each field:—Carbolic acid, sulphate of quinine, chloride of zinc, and sulphuric acid. The third class are those which acted injuriously on the vibrios on their addition, leaving only a small number retaining the power of swimming, but which allowed the vibrios gradually to increase in number, the fluid, nevertheless, containing less life after sixteen days than the standard putrid albumen-solution:—Picric acid and sulpho-carbolate of zinc. The fourth class includes those substances which acted injuriously at first, but permitted the vibrios to regain their former locomotive power, so that the fluid after sixteen days contained as much vibrio-life as the standard putrid albumen:—Chloride of aluminium, sulphurous acid and prussic acid. The fifth class contains those compounds which acted injuriously at first, destroying the locomotive power of most of the vibrios, but which afterwards permitted the vibrios to increase more rapidly than in the standard albumen-solution:—Bleaching-powder, bichloride of mercury, chlorine-solution, caustic soda, acetic and nitric acids, sulphate of iron, and the sulpho-carbolates of potash and soda. The sixth class contains those compounds which exercised no action on the animalcules, either at first or after sixteen days:—Arsenious acid, common salt, chloride of calcium, chlorate of potash, sulphate of lime, bisulphite of lime, hyposulphite of soda, phosphate of lime, turpentine, and pepper. The seventh class includes those substances which favour the production of animalcules and promote putrefaction:—Lime, charcoal, permanganate of potash, phosphate of soda, and ammonia.

Structure of Normal and Varicose Veins.—The 'Lancet' of July 13th states that Dr. S. Soboroff has lately undertaken a series of researches on the structure of healthy and varicose veins, the results of which possess considerable interest. He had at his disposal eight specimens of varicose veins, which, with only one exception, were accompanied by pathological changes in the vena saphena. These were all capable of being arranged in two groups: in both the lumen of the vessels was greatly enlarged; but in one the walls were thickened, in the other they were attenuated. The preparations seemed to have been made with great care, some being silvered to show the epithelium; others subjected to the action of chloride of gold, chloride of palladium, and chromic acid. The principal fact that he has definitely ascertained is that a dilated venous trunk, even when possessing almost perfectly transparent walls, contains colossal muscular fibre-cells. These are so large that when first observed they were regarded as fasciculi of unstriped muscle, but more careful examination, aided by staining experiments, showing the well-marked outline of the cells and the rod-shaped nuclei, clearly demonstrating that fibre-cells alone were under the microscope. The adventitia of the dilated veins was thin and but sparingly supplied with vessels, and the epithelial lining of these attenuated vessels was apparently continuous and unaltered. In the thickened and dilated veins, on the other hand, the adventitia was hypertrophied, and the vessels distributed to it were large and numerous.

A Fossil Hydractinia.—Professor George Allman, F.R.S., in a communication to the 'Geological Magazine' for August, says, that among the very few known instances of fossil hydroids, the genus *Hydractinia* must be included. Under the name of *Cellepora echinata*, M. Michelin has described a fossil from the Subapennine group of Asti, and from the Superior Fallunian of Bordeaux and Dax.* M. Fischer has drawn attention to the fact that the *Cellepora echinata* of Michelin is really a *Hydractinia* encrusting a *Murex* or a *Nassa*, while he has himself added another fossil *Hydractinia* from the Upper Greensand of Mans.† This he found in the collection of M. Alc. d'Orbigny, where it encrusted numerous specimens of *Natica tuberculata*, d'Orbig., from that formation.

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* Michelin, 'Icon. Zooph.,' p. 74, pl. xv., fig. 5.

† Fischer, 'Bull. de la Soc. Géol. de France,' 2me sér., t. xxiv., p. 689.

M. Fischer has assigned to this species the name of *Hydractina cretacea*, while to Michelin's species he has given that of *Hydractina Michelini*. To the two examples thus noticed by Michelin and Fischer, Professor Allman is enabled to add a third from the Coralline Crag of Suffolk. It occurs among some Coralline Crag fossils in the collection of the British Museum. It was found encrusting two specimens of *Purpura lapillus*, one from Orford, and the other from Gedgrave, in Suffolk. It covers with a continuous crust the shell over which it spreads, and has a minutely alveolar structure, with its surface thickly set with short blunt spines. The original chitine of the common basal expansion is entirely replaced by carbonate of lime. There cannot be the slightest doubt of this fossil being a true *Hydractinia*, and indeed it is impossible to find any characters which can separate it from the living *Hydractinia echinata*. From the mere fossilized basis, however, which is, of course, all that has come down to us, we should not be justified in asserting its identity with the living hydroid.

The Epitheca in Fossil Sponges.—Dr. B. Holl in an article of some length explains that in the fossil sponges some portion of the surface, especially externally towards the base, is frequently observed to be either without pores, or they are so minute as to be invisible, and the sponge then appears as though covered by a more or less smooth or slightly-wrinkled membrane, which has been regarded by D'Orbigny, De Fromentelle, F. A. Roemer, and others, as analogous to the epitheca of the *Zoantharia*; and the occurrence of this epitheca has been held to be an additional evidence of the stony nature of the sponge skeleton. When examined microscopically, by means of thin sections, however, it appears that this epitheca is due to the filling up of the interstices of the superficial parts of the sponge, which, in the situations in which it exists, is finer and more condensed than elsewhere. This greater density at the surface may be seen in many recent sponges, the superficial portions of the tissues being closer and finer than that of the interior, which was formed during an earlier and more active period in the growth of the sponge. But either from having arrived at maturity, or at a period when the growth was temporarily arrested—for in some sponges the growth is intermittent—or, as appears sometimes to be the case, from some local cause, the tissue at the surface assumes a closer arrangement. Thus in the common *Halichondria panicea*, the surface over greater or lesser portions frequently presents a condensed appearance with scarcely any visible interspaces, the outer superficial portion being made up of a densely-matted layer of spicula placed for the most part parallel to the surface; and the same is true of many fibrous sponges, as shown by Dr. Bowerbank. In some of the fossil sponges a similar modification of the tissue at the surface appears to have obtained, especially in certain cup-shaped and cylindrical sponges, and in the calcareous fossils in which this has been the case, the interstices, from their extreme minuteness, are more or less filled with carbonate of lime. Thus a species of *Cupulospongia*, common in the gravel-pits of Farringdon, frequently presents on its interior a smooth surface, described by the late Daniel Sharp as a membrane. But if a number of individuals of this species be examined, it will be observed that although it sometimes completely lines the interior of the cup, it more often occurs only in patches; and that, while some of the interstices are blocked up, there are others that remain open, and this not as the consequence of friction or weathering, but as the result of fossilization. It is, however, in some of the sponges of the Oolite that we see this infilling of the interstices most distinctly, in some of the cylindrical forms especially, the whole of the sponge, except its summit, appearing as though invested by a sheath, but which, were it really of the nature of a true epitheca, as in the *Zoantharia*, it would be difficult to comprehend how the functions of the animal were carried on. It is more than probable that this structure is nothing more than the cast of the impression or mould of the outer surface of the sarcode of the sponge, perhaps slightly thickened, but it is not constantly present even in the same species. For example, there is a small cylindrical sponge, not unfrequent in the Coral Rag, at Bullington Green, near Oxford, in which more or less of this so-called epitheca is met with in some individuals, while the greater number show nothing of the kind. It appears, therefore, that the epitheca is sometimes only a result of fossilization, and is sometimes probably the cast of the outer surface of the sarcode which has left its impress on the mould; that it is absent in the earlier and growing stages of the sponge, and is not constantly present in the matured individuals of those species in which it occurs; and, moreover, that it sometimes results from the contact of foreign bodies, in consequence of the increased density of tissue which such bodies are apt to produce. Its value, therefore, even as a specific character, is not great.

Microscopic Fossils in Irish Chalk.—At a meeting of the Royal Irish Geological Society, on June 12th, Professor Hull, F.R.S., exhibited two slices of Chalk from County Antrim, showing under the microscope its essentially foraminiferous structure. On comparing the forms with those in Ehrenberg's 'Mikrogeologie,' Professor Traquair and the author were able to identify at least five genera, namely, *Rotalia*, *Textularia*, *Planulina*, *Nodosaria*, and *Globigerina*—this last named being, as in the Atlantic mud, the most abundant; and, in fact, constituting the greater part of the mass of the rock. The cross-section of the spine of an *Echinus*, and other forms of doubtful affinities, were also observed. The shells were nearly all preserved in calcite, and bound together by impalpable carbonate of lime. The slices were made by Mr. Jordan, of the Museum of Practical Geology. Mr. Hull called attention to the fact that the Foraminifera of the Chalk of the north of Ireland had scarcely received any attention from palæontologists. This, he considered, might be partly attributed to the unusually indurated character of the stone, which rendered it almost impossible to extract Foraminifers by washing. This quality, however, was favourable to the preparation of thin slices for microscopic examination; and Professor Rupert Jones had kindly undertaken to examine slices of the Antrim Chalk for comparison with those forms now under investigation by himself and Mr. Parker.

Microscopic Organisms found on Inland Ice.—Dr. Berggren has communicated a notice of these organisms to the 'Geological Magazine,' August, and promises a fuller account in the 'K. Vet. Akademiens Ofversigt.' The notice constitutes part of Professor Nordenskiold's paper. It seems that one of the species of Algæ met with on the inland ice occurred in such vast quantities, that the surface of the ice throughout larger or smaller tracts was tinted with a peculiar colour. Two others seemed exclusively to belong to the fine sand, which is found either in the form of a thin covering on the surface of the ice, or as a more or less thick layer at the bottom of the pipe-like holes that appear in the surface. The first-mentioned species, occurring copiously, does not require any such substratum, but is found principally on the sides of ice-hills, where the water from the melting ice filtered itself out between the little inequalities of the surface. The most copiously represented species has the form of a short thread, not spreading out in branches, but consisting of a single row of cells; the number of cells in each thread is 2, 4, 8, or at most 16. Threads of 4 and 8 cells are most common. The species very frequently appears only as a single cell. The threads are usually a little bent, sometimes, when the number of cells is 16, forming a complete semicircle. The number two or its multiples taken as the standard for the number of cells in the separate threads is accounted for by the regular continuous bisection of the cells, whereby their propagation proceeds. The connection between the cells is the looser the older the partitions become, as the older membranes assume a looser consistence. In a thread of 16 cells, the connection between the eighth and ninth cells is soon broken, and in the two threads thus resulting the connection between the fourth and fifth cells is weaker than that between the second and third or the sixth and seventh. The threads therefore often lie bent at an angle. The diameter of the cells is 0·008—0·012 mm., and their length 0·016—0·040 mm. Individual cells may sometimes attain a length of 0·065 mm., and a breadth of 0·015 mm., whereas a great number of other single cells are met with of very small dimensions, from spherical forms of only 0·006 mm. diameter to those of ordinary form and size. As the ends of the cells, where they are joined together, are rounded, there is, of course, a contraction between them, which becomes more and more conspicuous as the connection between them is loosened by time. The membrane is thin and hyaline, and its outermost layer (the remnants of the membranes of the mother-cells altered after division) is of an almost slimy consistence, whereby the cells are for some time kept together. The contents of the cells are in part concealed by a dark purple-brown colouring matter, which in dried cells is immediately drawn out on wetting them. The centre of the cells is occupied by an oblong or cylindrical mass of chlorophyll, of somewhat irregular contour, in the extremity of which two kernel-formed rounded bodies are imbedded, which in general cannot be perceived by the eye till the colouring matter has been removed by means of reagents. We sometimes meet with four such bodies in one cell, sometimes but a single one: the former a result of accidentally checked division of the cells; the latter of such division having lately taken place. In the liquid of the cells a number of small grains are found, which are for the most collected round the periphery of the cell or at its ends. "Judging from the construction of the cells, and the manner of their multiplication, the alga before us would appear to belong to the *Conjugatæ;* but as I have not succeeded in discovering fructification in it, it would be rash to decide to which genus it is to be referred. The thread-like rows of connected cells agree with the *Zygnemaceæ;* whereas, on the other hand, an unmistakable similitude to the *Desmidiaceæ*, especially *Cylindrocystis*, and the nearly-related genera, is indicated by the strongly-marked divisions into multiples of two, and by the tendency of the rows of cells to fall asunder, as far as the destructibility of the uniting cell-membranes permits, into parts consisting of cells united in pairs, which however is seldom possible, in consequence of the greater energy possessed by the power of multiplying the cells. As the above-mentioned small single cells, which occur in great numbers, are much less in diameter than those cells which arise from the bisection of the threads, they have perhaps a different origin from these latter, although the researches which I have hitherto been enabled to devote to this subject have not furnished any illustration of it. Were these daughter-cells arising from the division of the sporæ, if the above-mentioned supposition with respect to the systematic place of the species be correct, the stadium of copulation or sporæ, in some period of its development, ought to be found. Two rare forms of peculiarly constructed cells perhaps ought not to be passed over in silence. I have sometimes found the extreme cell in a thread considerably more swelled than the others, more elliptic in form, also provided with a thicker membrane, and with the contents of the cell more coarse-grained. I once found one of the middle cells in a thread thus transformed, and on two occasions I have met with single cells of the same kind. I also once met with a cell of very peculiar construction. It had the usual form, but was unusually large, with a long mass of chlorophyll, as usual, in the midst, and the granular matter grouped rather towards the ends of the cell. In it were found about twenty larger or smaller spherical bodies. Four of these lay arranged at each end of the cell, and were almost entirely opaque, of a dark brown colour, and in appearance much resembled the smaller cells of *Protococcus nivalis.* The others were translucent, with sharply-defined contours. As our knowledge of the nature of these bodies is confined to what is here stated, the fuller explanation of their significance must be reserved for a future investigation. In places similar to those in which this species occurs, and often in company with it, *Protococcus nivalis* was met with. Amidst the fine gravel upon the ice, but to a trifling amount, small green cells are found, sometimes united in little groups, sometimes isolated, which appeared to belong to *Protococcus vulgaris. Scytomena gracile*, on the other hand, is everywhere met with in great profusion, wherever the gravel either lies in thinly-scattered grains on the surface of the ice or forms more or less thick layers. The threads lie either alone, or united in small bunches, as they join together at the lower part, and bend backwards higher up. They are pretty stiff, S-shaped, or forming a curve of several undulations, and yellowish brown in colour. Their length is very various, their breadth generally about 0·009 mm."

Observations on Collema.—These observations are continued by Mr. W. Archer in 'Grevillea' for August. The author concludes a paper of some length with the following three suggestions:—1. To suggest the possibility that, if we may conceive Dolichospermum, &c., excluded from the "gonidia question" as forming special fruit (that is, "spores"), so might we regard Nostoc as excluded, though its formation of spores be so extremely rare. Seemingly, indeed, the formation of spores by an algal species, supposed to become occasionally *lichenized*, is not a reason *against* the hypothesis as viewed by Schwendener—he only assumes that such an example of the alga surrenders, or leaves in abeyance, its tendency to the production of spores. 2. To suggest that there are veritable lichens which live submerged, and produce their apothecia. I presume, however, it might be replied that such may have received their inoculation by the parasite during some season of drought, when the *alga* lay "high and dry." 3. To suggest the possibility that the spores of Collema, if "sown" on some other gelatinous substratum, besides that of Nostoc—say, for instance, a Palmella or Mesotænium—might equally well germinate, penetrate therein, and develop a hypha. There seems, I venture to think, no *a priori* reason against this supposition—inside the Nostoc, the "reserve-stuff" of the spore being exhausted, and the chains of Nostoc filaments admittedly intact, the only next immediate source of nutriment for the growing hypha would, I imagine, in the experiment of Reess, appear to have been the "Nostoc-jelly.". Now a "Palmella-jelly," or a "Mesotænium-jelly" (both aërial, that is, not under water), would seem in themselves to be possibly just as likely to afford the requisite *pabulum* for the germinating and growing Collema-spore. If this conjecture should be borne out, which I would indeed put with all diffidence, what would be the result of Reess's experiments, or, rather, what proven thereby? Such a combination (*if* capable) with a Palmella or a Mesotænium would not be "Collema," because it would not have "nostochaceous" gonidia, nor the characteristic periderm. If, indeed, we might for a moment assume that which direct experiment alone could prove, and a germination of spores and penetration of the hypha of Collema with a Mesotænium effected, such a "lichen-thallus" would be, I apprehend, unprecedented—a hypha *like* other lichen-hyphæ, no doubt (but *known* to be that of a Collema), with large elliptical or cylindrical "gonidia" containing a central "chlorophyll-plate," and which would probably (in free nature at least) go on and produce *zygospores!*

NOTES AND MEMORANDA.

Death of Dr. Stimpson.—We regret to learn from 'Silliman's American Journal' for June, of the death of Dr. William Stimpson, which occurred in the evening of the 26th of May last. He had recently returned from a dredging expedition across the Gulf of Mexico, and had been very ill since his return. He has for many years been well known as one of the leading zoologists of this country, and has devoted himself chiefly to marine invertebrata.

What Work can be done with the Microscope. — Mr. Charles Stodder writes to the 'Boston Journal of Chemistry,' June, to explain how an observer may study fermentation under, or rather over, the microscope. He says that the work of "breaking up organic compounds" can ever be understood, explained, or seen under the microscope, is very problematical, but as yet there is no evidence before the public that the research has been conducted with the best modern resources of the microscopist. With one instrument of investigation, the inverted microscope, the object will be not under the microscope, but the microscope under the object. With this instrument the evolution of the gas will interfere much less with the observation of the processes going on. Then, most of the published accounts of investigations of this kind, in France and Germany, appear to have been made with instruments of the construction of ten, fifteen, or twenty years ago, with powers of 400 to perhaps 1000 diameters. Now, *immersion* objectives of $\frac{1}{16}$ to $\frac{1}{50}$ of an inch focus, giving with ease powers from 1600 to 10,000 diameters, are in constant use. It is not known that such instruments have ever been applied to this class of studies. They have given remarkable results in other investigations, and until they have been applied to the yeast plant, "morbid or germinal growths," &c., we do not and cannot know all that may be learned about them. It is with such instruments of the better quality that these organisms will have to be studied. Interest in microscopy is rapidly growing in this country. Hundreds of young men have procured their instruments. Here is a branch in which almost everything is yet to be learned, and fame and honours will be the reward of those who add to human knowledge. Very few European microscopists, when publishing their results, ever describe the instruments they work with; as a general thing they merely say the object was magnified so many diameters, forgetting, or often not knowing, that 500 diameters with one instrument may be vastly inferior or superior to the same power with an instrument by another maker. Astronomers always record and publish the instrument *their* observations are made with. The microscopist ought to do the same. He should give the maker of his objective, its angular aperture, and *real focus*, not merely what the maker sold it for; for it is now a well-known fact that the instruments of some of the most renowned makers are really very different things from what the makers call them. In fact, in the words of Dr. W. B. Carpenter, "$\frac{1}{4}$'s are often $\frac{1}{5}$," and in the words of Mr. F. H. Wenham, "$\frac{1}{8}$'s are often $\frac{1}{9}$ or $\frac{1}{10}$, and sometimes $\frac{1}{12}$!" Thus the makers may obtain a reputation for the performance of a low power to which they are not entitled.

The Hunterian Museum and its Microscopical Collection.—In his annual report to the Council of the Royal College of Surgeons, Professor Flower, the Conservator, states that the additions to the Pathological Collection for the past year have been eighty-eight in number, as compared with sixty-two during the previous year, and that the microscopical characters of all recent specimens sent to the College and thought worthy of preservation have been carefully described and delineated by Mr. Goodhart, the Pathological Assistant, who, by the great zeal with which he has performed the duties entrusted to him, appears fully to deserve the eulogium of the Conservator.

CORRESPONDENCE.

[By a mistake of the printers, Mr. Tolles' first letter was "crushed out." We now give it with his second one.—Ed. 'M. M. J.']

Angular Aperture of Immersion Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *June* 25, 1872.

Sir,—I wish to announce that I have lately subjected high-angled objectives to the tank method proposed by Mr. Wenham for ascertaining angular aperture, the objective being immersed (front surface dipped) in balsam. The result is very different from that which Mr. Wenham declared must happen.

Let this note appear, if possible, in your August issue proximo, and I will describe my results and method particularly in ample time for appearance in your Monthly for September next.

With my account I will send *an objective*, that my claimed results may be verified or disavowed.

Very respectfully,

Robt. B. Tolles.

To the Editor of the 'Monthly Microscopical Journal.'

Dr. Henry Lawson, Boston, *July* 15, 1872.

Sir,—In my note of June last, I declared my purpose to send an objective for test of angle in balsam.

Professor Markoe, of this city, sails to-morrow, per steamer 'Hecla,' from this port; and as an objective suitable to my purpose, made in 1869, has been very obligingly placed at my disposal by another gentleman of Boston, I venture to forward it to your care.

Without any wish to thrust the matter upon Mr. Wenham's further attention, I still would like Mr. W. to be notified that the objective is forwarded for the purpose of trial by him of its angle in balsam.

Adjusted at "10½" closed from open point (which is the intermediate point of the whole adjustment), I make the angle in balsam, using the tank as proposed by Mr. Wenham, 93°, or possibly 95° by sunlight.

With a view to help to a conclusion, perhaps you will have no objection to giving Mr. Wenham notice of receipt of the objective, and that it is for his use, for the purpose aforesaid, if he chooses to subject it to the test.

N.B.—I easily remove the balsam with a little cool oil, and no harm to lacquer or glass.

With much respect,

40, Hanover Street (Room 30). Robt. B. Tolles.

July 19*th*, 1872.

P.S.—Further remarks on account of experiments on angular aperture shall be forwarded per steamer of 23rd inst. from this port. —R. B. T.

[The Editor begs to say that he has not received any objective from Mr. Tolles up to the date of August 13th.]

PROCEEDINGS OF SOCIETIES.

Oldham Microscopical Society.

The annual meeting of the above Society was held at No. 2, High Street, in May last. The Secretary reported that last year the Society had 23 members, and a balance of 1*l*. 15*s*., whilst this year it had increased to 43 members, and a balance of 6*l*. 10*s*. 6*d*. The librarian's report stated that the Society possessed 21 vols. of standard works on the microscope and the various scientific subjects to which it has been applied, and it was decided to expend the balance in the purchase of further standard works of approved excellence. Dr. Thomson was elected President for the ensuing year, and officers to serve under him were also elected. The Secretary submitted the following programme for the coming year:—Four excursions, available for members and their friends, during the months of June, July, and August, *viz.* to Tandle Hill, Marple, Littlebro', and one in connection with the Scientific Students' Society, of Manchester, place not decided. Papers on the following subjects, *viz.* "Pond Life," Mr. R. Horne; "The Microscope as Applied to the Detection of Disease," Dr. George Thomson; "Animal Tissues," Dr. Fawsitt; "Reproductive Organs of the Lichens," Mr. Charles

Walters; "Lichens," Mr. Brittain, of Manchester; "Fungi," Mr. Pullinger; "Fossil Botany," Mr. J. Nield; "Internal Structure of Fossil Plants," Mr. J. Butterworth; "Vegetable Tissues," Mr. J. R. Byrom; "Closing Address," the President. The retiring President, Mr. James Nield, read a very able *résumé* of the Society's proceedings during the past year, of which the following is an abstract:—After congratulating the Society on its position, financially and otherwise, and adverting to its excursions, he passed to what he described as the real work of the Society, namely, the useful series of papers delivered during the session, and which he characterized as being every way worthy of the Society over which it had been his honour to preside. Mr. Horne's very interesting paper, given in October, "On Infusoria," was a valuable acquisition, and invested those lowliest of beings which people our ponds in such myriads with an interest and importance which rendered the subject a most pleasant and profitable one to investigate. He would therefore pass on to that given in December by Dr. Fawsitt, "On Blood," illustrated by diagrams, and prepared slides of blood corpuscles, exhibiting many modifications of those disk-like living bodies as they occur in various types of animal life, and likewise of those singular objects, "blood crystals," which result from a breaking up of the red corpuscles, and which in the human blood take a prismatic form, and in other creatures lower in the scale of being, modifications of the prism. The paper was of an exhaustive character, as far as scientific research has hitherto enabled the student to advance. One of the results of this Society, numbering amongst its members several gentlemen holding high and honourable positions in the medical profession, was made manifest in this paper. Dr. Fawsitt brought his professional knowledge and experience to bear on the subject of his paper, adding a medico-physiological feature to its other important peculiarities. The legitimate work of the microscopist is, undoubtedly, practical and utilitarian in character. Yet the student can rightly claim to enter into the realms of theoretical speculation. Dr. Fawsitt prudently, I think, confined himself principally to ascertained facts. He made no attempt at generalization, or from an aggregation of accepted data to make abstract deductions. Yet the moment was propitious. And let me admit that during the progress of the reading, with the diagrams and prepared slides of blood and its crystals before me, the complete, or nearly complete, identity in form and chemical composition of the blood of all the vertebrata, and the intimate connection and relationship of all these varied animal organisms, seemed to me to be clearly established to the extent of the subject of the paper. When we look upon the colourless corpuscles of the human blood, and remember their chemical composition, their ever-changing form, and the purpose they serve in the economy of the human body, the conviction is forced upon us that man, the highest and grandest work of the Creator, is connected by these peculiar links in the chain of being with the humble and lowly rhizopoda, the latter finding congenial conditions in water, the former floating in the plasma of the human blood. The naked and protean amœba, and the sarcode clothed in ornate foraminiferous test, are but free members of a family, which are aggregated and communistic in the sponge, and in the blood of man, and all intermediate creatures. "The Principles Involved in the Construction of the Microscope" was the subject of our next paper, and was dealt with by Mr. Waddington, in January. It seemed to me at the time that this was a very proper subject to bring before our Society, as many of our members, myself among the number, are insufficiently informed in the practical application of the microscope to the study of our department of natural history; still less are we acquainted with the optical principles involved in the construction of that all-important instrument. Mr. Waddington, who has made the microscope his study during many years, gave us the benefit of his extensive knowledge, and the result was a clear and instructive paper on the theoretical principles which find their expression in the microscope. There is, perhaps, no department of science that has made more important progress, in so short a space of time, as the study of the theory of light as involved in the manufacture of object-glasses, and the actual building up of the instrument itself. The subtle intellect and practical genius of the various makers, both Continental, American, and English—and pre-eminently that of Messrs. Powell and Lealand, of London, aided by the wonderful manipulative skill of Col. J. J. Woodward, of America—have been concentrated on this labour, and the results have been the exploding of ideas and errors that have nothing to recommend them but their age. But a few short years ago the resolution of lines, now rendered easy of accomplishment by our cumulative knowledge and practical thrift, would have been considered, and were, beyond the limits described by our then knowledge of the principles of light. In February there was an unavoidable hitch in our programme, and no paper was ready or work recorded in the Secretary's minutes. In March, however, Mr. Walters gave us a very able paper "On the Reproductive Organs of the Fungi." This was very properly followed, in the month of April, by an equally valuable one "On Microscopic Fungi" by Mr. Pullinger. As may be seen by the titles of the papers, each gentleman directed his labour to a separate part of the same very difficult subject. The undertaking was a formidable one, and no doubt taxed their mental energies, and drew largely on their botanic lore for fungal histology. I may venture to say, without fear of contradiction, that their ability proved itself to be equal to their self-imposed task. The study of fungi is still a difficult one. Many facts in their life-history are yet wanting. Many phenomena exhibited by them remain unexplained, and much labour and patient investigation are required to elucidate their manifold wonders. Our vision is limited, the field is large, the horizon is clouded, and many phenomena are dim and ill-defined in outline. The neglect in which the study of these objects has been left is doubtless explained by their apparent want of those points of beauty of form and colour which render some other departments of botanic study so attractive. Many, too, are difficult to approach on account of their essentially microscopic character, yet botanists who have made these objects the subject of their earnest inquiry aver that they are most interesting histologically, and that they possess a fair share of beauty as understood in an æsthetical sense. Apart, however, from the difficulty attending their study, and the repulsiveness and superstition with which ignorance has wrongfully invested them—apart from their want of beauty or otherwise—it is of vital importance to man, in an economical sense, that these hitherto obscure organisms should be understood, in order that they may be combated. "Their name is legion." Their power for good or evil is immeasurable. Between them and man the battle is chronic. The struggle for existence and ascendency is increasing as man becomes more civilized and varied in his tastes. No object of man's solicitude is safe from their attack. The cottage and the palace are equally in danger. The stores of priceless treasures that the latter contains form no ground of exemption from ruin. Man's choicest works of art, upon which his brain has laboured and his wealth been lavished, are marked by the fungi, as a sheriff's officer marks an insolvent debtor's goods, for destruction. Thousands of objects, whether placed high in the air or deep down in the soil, are still within the reach of this ubiquitous race. Some spread themselves over our fruits before we have time to gather and use them; others fill the cracks and crevices of our bread and cheese with the pellucid forests of pigmy trees, bowed down with the abundance of the fruit they bear. Our jams, pickles, and preserves, if not watched and defended with the greatest care, at some seasons are immediately stamped with their broad arrow as their own. "When our beer becomes mothery," says Dr. Badham, "that mother of mischief is a fungus. If pickles acquire a bad taste, if ketchup becomes ropy and putrefies, funguses have a finger in it all. Their reign stops not here, they even prey one on another. The closed cavities of nuts afford concealment for some species; others, like leeches, stick to the bulbs of plants, and suck them dry. Some pick timber as men pick oakum. More than one species has a fancy for the hoofs of horses and the horns of cattle, sticking to them alone." The viscera of our domestic fly is liable, in autumn, to break out into vegetable tufts of fungous growth. In spring I have myself disinterred numerous caterpillars with a species of clavarias larger than themselves growing from their heads. And the microscopist has often to lament that one or more of his once most valuable objects are, by the depredations of some species, rendered quite worthless. Thus we see that man cannot well treat with indifference these simple living structures. They are as varied in form as they are ubiquitous in habit. They are as dangerous from their numbers as from the insidiousness of their attack. Man's house, his clothing, and his utensils; the air he breathes, the water he drinks, and the food that he eats are claimed by fungi, in some shape, as their inheritance; nay, they even enter his mouth, and settle upon his teeth, thus boldly putting their heads into the lion's mouth. Mr. Walters and Mr. Pullinger are the Castor and Pollux of our Society, and, armed with the microscope, and that still more potent weapon, human thought, they have taken to the field against their numerous foe, and with twin-like unanimity they have undertaken to reconnoitre and examine the enemy's position and strength, to know and to conquer him. In concluding, let me thank

you, the members generally, for your regular attendance at the monthly meetings, and for your patient attention during business, which has rendered my duties as President light and easy; and let me thank the retiring officers specially for the manner in which they have fulfilled the duties immediately under their care, and assisted me in my pleasant labours, during the official year which this evening brings to a close.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.*

February 15th.—Microscopical Meeting. Mr. Hollis, President, in the chair. Mr. T. H. Hennah read a paper "On the Palates of Mollusks."

Mr. Hennah prefaced his remarks by stating that he should regard the palates of mollusks as objects especially calculated to foster a true spirit of microscopical inquiry. They might be briefly described as membranes, studded more or less with teeth, found in the buccal cavity of univalve and shell-less mollusks. While the arrangement of the teeth was always symmetrical, the palates appeared to fulfil several functions, in some mollusks acting as a gizzard only, being supplemented by strong cutting mandibles, as in snails, cuttle-fish, &c.; in others acting both as a rasp and as a gizzard. In some the provision for wear and tear was prodigious: the palates of the limpet and periwinkle being longer than the animals themselves; while in other cases they were comparatively small and out of proportion, as compared with the animals. Thus the palate of the cuttle-fish was about the same size as that of the common snail.

As elsewhere, in the animal kingdom, specially-adapted contrivances were found, so also was the palate of the mollusk adapted, in its form, position, and arrangement, to the special habits of each species. In this variety, apart from its scientific interest, lay an inducement to go on with the examination when it was once begun. He well remembered dissecting out a palate of a common snail—his first dissection and almost his first microscopic preparation—and he could safely say he was grateful for the influence on his pursuits thus exercised by the silent tongue of this poor snail. Whelks, periwinkles, slugs, &c., were its successors, and though he found each had its palate covered with brilliant teeth, different methods of dissection were required for each; and thus a foundation was laid for a knowledge of the comparative anatomy of different species of the same family. He thought nothing gave a sounder teaching in Natural History than the thorough examination of any one organ, such as the "lingual membrane" of the mollusca. The variety was very great, but yet there was a family likeness running through the palate of many allied species.

Attempts had been made to base a classification of the univalve mollusca on the form and disposition of the teeth on the lingual membrane. Other characters of greater prominence had been found, however, to be inconsistent with its complete adoption, and although of much value in assisting the definition of species, having little external to indicate their difference, the dentition of the mollusca occupied a subordinate place in their classification. At the same time it was so characteristic, that any definition of species must be held to be imperfect without a figure or formula expressing the arrangement of the teeth. Instead of making excerpts from books, or merely quoting the systematic classification of lingual membranes which had been adopted, he would mention some books in the library containing the best information on the subject:—'Microscopical Journal,' vol. i., p. 170, 1853, "Teeth and Tongues of Mollusca," by Dr. J. E. Gray; 'Intellectual Observer,' vol. v., p. 67, 1864, "Dentition of British Mollusca," by Rev. G. Rowe; 'Transactions of Royal Microscopical Society,' July and October, 1868, "The Lingual Membrane in Mollusca, and its Value in Classification," by Mr. Jabez Hogg.

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* Report by Mr. T. W. Wonfor.

He strongly advised *dissection* as the means of obtaining the palates, as the experience so gained would be valuable in other investigations; but there was a shorter method, which he had been obliged to adopt with such small animals as *Balea fragilis* (about $\frac{1}{8}$th of an inch long), or with larger, when many had to be prepared. It was simply to cut off the head, or even to take the entire animal, and boil it in *liquor potassæ*, when all the soft parts would readily dissolve away, leaving the palate, easily to be recognized by its brilliant teeth, separate and entire, and requiring only to be well shaken with water in a test-tube to free it from extraneous matter. After well washing, the palate should be placed in diluted glycerine before mounting in glycerine jelly, the best medium for the purpose. He had brought down a good series of palates for examination, and would dissect for practical demonstration the palates of such mollusks as he had been able to obtain.

Mr. Wonfor, while thanking Mr. Hennah for his paper, was glad he intended giving practical instruction in dissection, for he was indebted to him for a valuable lesson in palate dissection some years ago; in fact, a common whelk, dissected on that occasion, and which he had brought with him, made for Darwin, as one tooth in each row had a double instead of a single crown. Some eight years ago he communicated Mr. Hennah's plan of boiling in potash to the 'Intellectual Observer,' giving him credit for the simple plan of readily obtaining palates; and was amused, some five years afterwards, to see it quoted from an American journal as a novel American idea. He mentioned this, for whatever credit was due for the suggestion belonged to Mr. Hennah.

The meeting then became a conversazione, when Messrs. Hennah, Glaisyer, and Wonfor exhibited numerous palates, including those of the whelk, limpet, periwinkle, snail, black slug, a vegetable feeder, testacella, a carnivorous slug, trochus, haliotis, cuttle-fish, aplysia, coris, clausilia, *Balea fragilis*, *Cyclostoma elegans*, &c.

During the evening Mr. Hennah gave practical instruction, by dissecting out the palates of snails, whelks, and periwinkles.

March 28th.—Microscopical Meeting. Mr. J. J. Sewell, Vice-President, in the chair.

Mr. Wonfor announced the receipt, for the Society's herbarium, from Mr. G. Davies, of another lichen new to Sussex, *Lecidea mœstula*, found by that gentleman near Billinghurst, and previously found by the Rev. J. Crombie near Lyndhurst, New Forest; and for the cabinet, from Mr. T. H. Hennah, of the palate of *Tritonia Hombergii*, said by Forbes and Hanley to be rare. A couple were taken alive, inside a scallop, to Mr. Hennah, who supposed they were dredged up with the scallops, as they were said to be inhabitants of deep water, and to frequent scallop-beds.

Votes of thanks were given to the donors.

Mr. Hennah, remarking on the minutes of the previous meeting, said he wished to qualify what he had stated about the potash mode of obtaining palates, which was admirable when comparison or identification was the object, but for palates which were to be mounted, no plan was better than dissection.

Mr. Wonfor announced that their friend Mr. T. Curties had very kindly sent down for examination a box of slides, including some very choice diatoms, mounted by Möller, and which were from a collection of Möller's, from Baker's, Holborn. He had brought down for distribution a number of specimens of *Cyclostoma elegans* and *Clausilia nigricans;* a couple of packets of loam, one from above and the other below the Wealden building-stone, and sent to Mr. Onions by Mr. Grantham, who wished some of the microscopists to examine it for vegetable substance. That afternoon he had received a letter from Mr. Gwatkin, and a bottle containing living creatures taken from the supply cistern of the kitchen boiler of Mr. C. Buckwell's house. It appeared "that since an alteration in the connecting pipe of the boiler and supply cistern, by which the temperature in the latter was reduced to lukewarm, these creatures from time to time made their appearance. The cistern was of cast iron, with tightly-fitting metal top. At first a little fine dust was seen on the surface, and, in a day or two after, the creatures themselves." Before coming down he had examined some, and found they were a species of Thysanuridæ (Spring-tails), which lived on the top of water. He fancied they were conveyed to the supply cistern from without. If the members present would look at one under the microscope, they would see the peculiar spring under the tail, whence the name. One evening in the previous week, while examining "battledore" scales, to see whether he could confirm Dr. Anthony's idea of the marking being tubercles on the ribs, he fortunately got some scales to stand on end, when he was able to see the tubercles standing out clear from the ribs. He had made a cork model of what he had seen, which illustrated their knobs, and pedicles, but not their bases. He was satisfied Dr. Anthony was right in the main; but scales taken from fresh-killed specimens should be examined, as in drying they had a tendency to become flattened.

Mr. F. E. Sawyer exhibited a block of indurated Portland stone, which was found by a mason in cutting a stone step, and pronounced by him to be a flint. He showed it to Mr. Wonfor, who considered it indurated Portland stone. With a lens its oolitic nature could be easily made out. He had prepared some of the loose chalk-like particles, and had obtained portions of corals and sponges.

Mr. Howell exhibited disintegrated granite, from the shores of Lake Erie, under the form of fine sand.

The meeting then became a conversazione, when, in addition to the objects mentioned above,

A number of very interesting objects were exhibited by Messrs. Sewell, Hennah, F. E. Sawyer, R. Glaisyer, and Wonfor.

QUEKETT MICROSCOPICAL CLUB.

The Seventh Annual Meeting was held at University College, July 26, 1872, Dr. Lionel S. Beale, F.R.S., President, in the chair. The report of the Committee, read by the Secretary, congratulated the members upon the satisfactory condition of the club, detailed its progress during the past year, showed the number of donations made to the library and cabinet, with the number of members on the list; and referred to the publication of catalogues of the slides and books, the 'Quarterly Journal,' the excursions, the annual soirée, and the special services rendered by certain gentlemen named. The Treasurer's report showed a satisfactory balance-sheet, from which it appeared that the income from all sources had been 272*l.* 5*s.* 1*d.*, and that there was a balance in hand of 12*l.* 7*s.* 2*d.* Unanimous votes of thanks were passed to the Council of University College for the highly-valued privilege of meeting in the library of that building free of charge; to the President, for his valuable services during his two years' term of office; and to the Committee for their efficient conduct of affairs. The President then read the Annual Address, which was listened to with the greatest attention, and enthusiastically applauded by the meeting. The proceedings terminated with the election of officers and the installation of Dr. R. Braithwaite, F.L.S., F.R.M.S., as President of the club for the ensuing year.

MARGATE MICROSCOPICAL SOCIETY.*

The June meeting of the Society was held at the Church Institute Margate, on the 4th instant, the President, Dr. Pittock, F.R.M.S., in the chair.

Some donations to the library and cabinet having been announced, and votes of thanks accorded to the donors, an animated discussion took place on the paper read before the Society by Mr. B. F. Kyngdon at the last monthly meeting "On Parasites," and this was followed by the reading of a paper by the President "On Pollen," amply illustrated by mounted slides and diagrams.

The members of the Society have recommenced their occasional excursions in the Isle of Thanet. A very pleasant one took place on the 8th instant, when some of the chalk rocks were visited, and many objects of interest collected. A pond collecting excursion to the neighbourhood of Stourmouth was announced for an early date. We understand that the President and members are at all times happy to meet with members of other microscopical societies who happen to be visiting at Margate, and any information as to excursions, &c., will be gladly afforded on application to Mr. W. F. Hunter, Hon. Sec., or to Mr. W. Lane Sear, Hon. Librarian, who has placed his drawing office (he is a local architect) at the disposal of the Society, and the members meet there weekly to work with the microscope and mount specimens.

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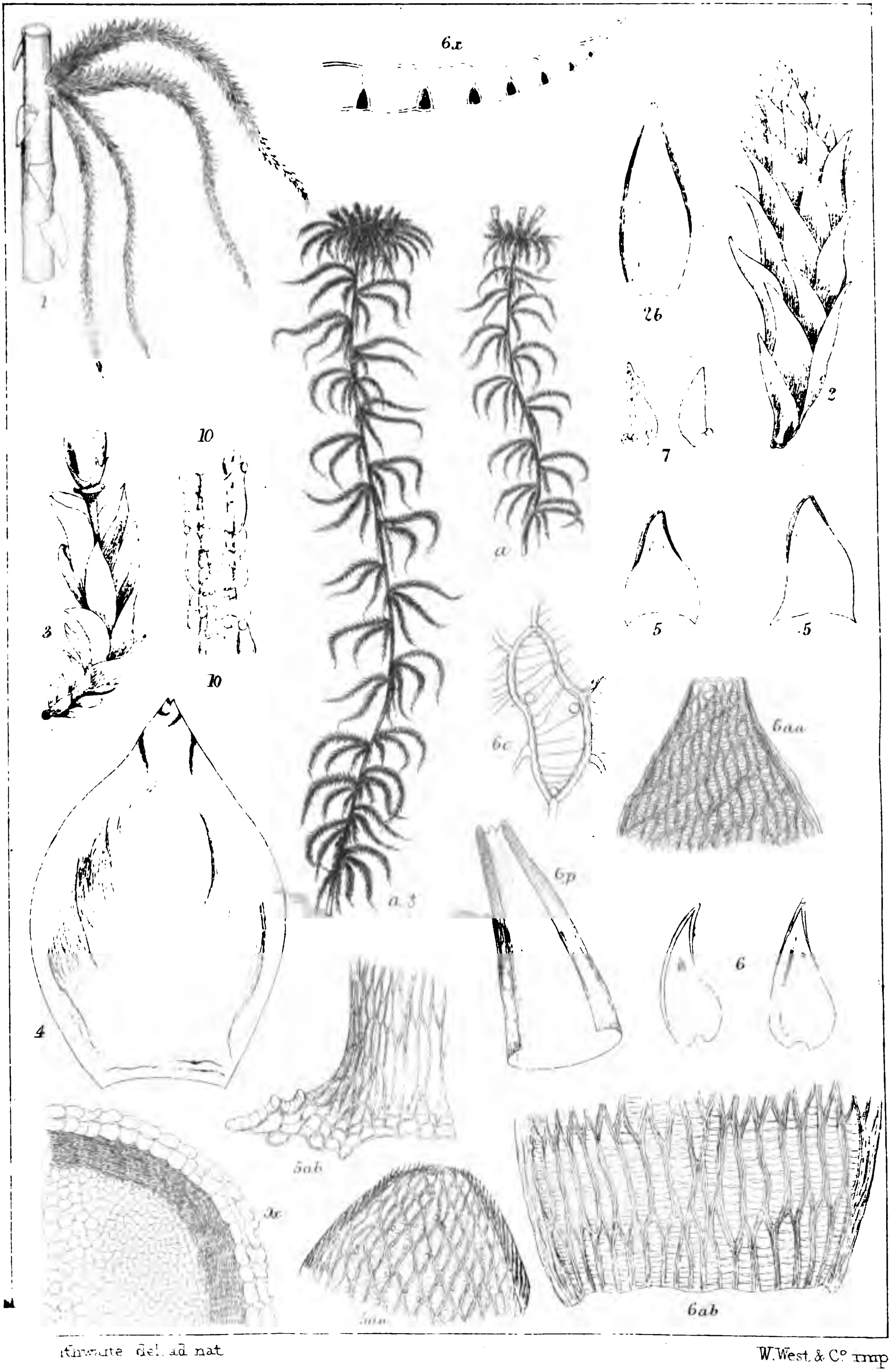
* Report furnished by Mr. W. Lane Sear.

The Margate Microscopical Society has been two years in existence, and numbers above 40 members.

The Monthly Microscopical Journal Octr. 1 1872.

Pl. XXX



Braithwaite del. ad nat.
W. West & Co. imp.

Sph. neglectum

THE
MONTHLY MICROSCOPICAL JOURNAL.
OCTOBER 1, 1872.

I.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

4. Sph. neglectum Ångstrom.

Öfver. Vet. Ak. Förhandl. XXXI, p. 201 (1864).

Plate XXX.

Syn.—Lindberg in Journ. Lin. Soc. XI, p. 468 (1870). Hartman, Skand. Fl. 10th edit. v. II (1871). *S. subsecundum* C. Müll. Synop. v. II, p. 539; the American specimens from Sullivant. *S. contortum* Sulliv. Mosses Un. St. p. 11, No. 3 (1856). *S. curvifolium* Wilson MS.—Hunt, Mem. Lit. and Phil. Soc., Manchester, Ser. 3, v. III, p. 233 (1867).

Dioicous.—4–6 in. high, pale yellowish brown, the apex yellow-green. Stem solid, simple or forked, the *bark pale, of* 2–3 *layers of cells.* Branch fascicles more or less crowded, of 3–4 branches, of which 1–2 are divergent, the others pendent, but not appressed to stems, not much elongated, nor acute. *Cauline leaves small, ovate, cucullate at apex, at last minutely fimbriate,* patent or reflexed, the basal hyaline cells utricular, the middle and lateral very narrow, the apical rhomboid, with scarce any fibres or pores.

Ramuline leaves laxly incumbent, *subsecund, recurved at point,* especially on the lower part of the divergent branches, ovate, shortly and bluntly acuminate, the point with 3–5 teeth, margin incurved in the upper $\frac{1}{3}$, rather broadly bordered; hyaline cells serpentine, elongated, filled with annular threads, pores many, small, arranged

EXPLANATION OF PLATE XXX.

Sphagnum neglectum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Capsule and peduncle. 4.—Peduncular leaf.
5.—Stem leaves. 5 *a a.*—Areolation of apex. 5 *a b.*—Areolation of base of same.
6.—Leaves from middle of a divergent branch. 6 *a a.*—Areolation of apex. 6 *a b.*—Areolation of base of same. 6 *p.*—Point of ditto. 6 *c.*—Cell from middle × 200. 6 *x.*—Section of same.
7.—Intermediate leaves from base of a divergent branch.
9 *x.*—Part of section of stem.
10.—Branch denuded of leaves.

principally at the outer wall of the cells, chlorophyll cells trigonous, projecting between the hyaline cells at the back. Basal intermediate leaves broadly bordered, with small rounded auricles. Retort cells of the branches straight, without any projecting neck. Catkins of male flowers subclavate, the bracts slightly secund, oblong, truncate, with several obtuse teeth, narrowly bordered, the areolation as in the branch leaves. Fruit seated in the coma, but little exserted, *peduncular bracts oblong, scarcely bordered, innermost large, convolute, emarginate,* the hyaline cells rather small, lower narrow, upper suboblong, without many threads or pores.

Hab., deep bogs, usually with *S. subsecundum,* for which it has probably been often overlooked.

Near Holyhead and at Vale Royal, Cheshire (Mr. Wilson), Loch Kandor, Braemar, 1868 (Mr. Hunt).

Closely resembling some forms of *S. subsecundum,* but distinguished by the pale bark of usually two layers of cells, and by the peduncular bracts; the fruit, however, is very rare, and the drawing is from an American specimen, collected in Cranberry Marshes, Northern Ohio, by Sullivant in 1849, for which I am indebted to my valued friend Prof. Lindberg. The male plant is from specimens communicated by the late Mr. Wilson.

II.—*Reply to " Further Remarks on Tolles' ⅛th and Powell and Lealand's Immersion $\frac{1}{16}$th."*

By Dr. J. J. Woodward, U.S. Army.

The July number of this Journal contains (page 13) an article entitled "Further Remarks on Tolles' ⅛th and Powell and Lealand's $\frac{1}{16}$th" (by Mr. E. Bicknell), which is so full of erroneous statements with regard to some of my work, that I cannot allow them to pass uncontradicted.

I do not feel called upon to enter into a discussion of Mr. Bicknell's arguments, but merely discharge the disagreeable duty of showing that his premises are not true.

He gives measurements of two of my positives on glass, each of which represents two frustules of *Amphipleura pellucida,* the one made by the first, the other by the second of the objectives above named. To these positives certain magnifying powers were attached.

He then proceeds to assume that the two positives were taken by each objective, without eye-piece, at 48 inches distance, and that the frustules represented had each 95 striæ to the $\frac{1}{1000}$th of an inch. In fact, neither assumption is true. The positives were copied from the original negatives in an ordinary copying camera. When I sent them to Boston, I wrote to Mr. Tolles that they were, as compared with the negatives, "enlarged a little in the copying camera." I also informed him that the frustules represented, counted in the microscope, the one 92, the other 93 striæ to the $\frac{1}{1000}$th of an inch. (This letter was sent July 10th, 1871.) Nothing was said then or since of the distance at which the original negatives were taken, and the powers of these original negatives were very different from those of the positive copies.

Now, the argument of the article referred to is based on three suppositions:—

1. That the positives are of the same size as the original negatives. 2. That these were each taken by the objectives named, without eye-piece, at 48 inches distance. 3. That the frustules represented, counted each 95,000 striæ to the inch. Each supposition is untrue. Perhaps the writer of the article may be able to explain why he made these suppositions; I, for one, would be willing to judge his excuses without prejudice.

Whether now his measurements of the positives are correct, is another question, and one which does not affect his argument in the least, since the size of the two bears no uniform relation to the magnifying powers of the two objectives at equal distances. I will only say that, as the positives were copied for me by another person, it is quite possible that even those from the same negative are not of the same size, though I intended they should be.

Mr. Bicknell's measures of the two he examined do not correspond with mine, of my retained copies, but I do not emphasize this point because it does not bear on the questions involved. The positives were sent out to show the relative defining powers of the two objectives, and not to indicate their magnifying powers; everything therefore was made subordinate to the first object, and as I did not measure each copy, I may perhaps have unintentionally fallen into error as to the size of some of these.

This possibility of erroneous figures as to some of the positives, does not apply to my published measurements of the actual magnifying powers of the objective in question; and as both glasses are still in the microscopical collection of the Army Medical Museum, I should be happy to give any *respectable* microscopist an opportunity to compare the two objectives as to magnifying power and other qualities.

III.—*On the History, Histological Structure, and Affinities of Nematophycus Logani,* Carr. (*Prototaxites Logani,* Dawson), *an Alga of Devonian Age.* By Wm. Carruthers, F.R.S., &c.

Plates XXXI. and XXXII.

Sir Wm. Logan, in his 'Report on the Progress of the Canadian Geological Survey for 1844,' published an account of his discovery of fossil plants in beds of Devonian age at Gaspé, in Lower Canada. In 1856 Dr. Dawson described, in the 'Proceedings of the American Association' (p. 174), one of the most interesting of these fossils—a stem which he considered exhibited a very remarkable and previously undescribed coniferous structure. With the view of more thoroughly investigating Logan's discovery he visited Gaspé in 1859, and published in the 'Quarterly Journal of the Geological Society'* a lengthened description of the plants. Among them was the stem already referred to, for which he established the genus *Prototaxites.* The description of the fossil then given is as follows:—"Woody trunks with concentric rings of growth, and medullary rays. Cells of pleurenchyma scarcely in regular series, thick-walled and cylindrical, with a double series of spiral fibres. Disk-structure indistinct in the specimen observed." The name was given to the stem because of the "spirally-marked cells characteristic of the genera *Taxites* and *Spiropitys* of Göppert, but differing from any conifer known to me in the cylindrical form and loose aggregation of the wood-cells, as seen in the cross-section." In the lengthened description Dr. Dawson says that in longitudinal section "the wood cells are seen to be much elongated, and to terminate in conical points, and their sides are covered with the remains of a double series of spiral fibres, among which are a few scattered roundish spots, which perhaps indicate a single row of disks." No pith is perceptible, and the medullary rays are marked by clear, structureless spaces, the cells having been entirely disorganized; but the

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EXPLANATION OF PLATES XXX. AND XXXI.

Plate XXXI.—Containing transverse sections of *Nematophycus Logani,* Carr. Fig. *a*, showing across the centre of the specimen the tract of smaller cells, which gives the exogenous aspect to the stem; and an open space with a few scattered tubes,—a medullary ray of Dr. Dawson. Fig. *b*, a part of the open space in *a*. The artist has produced the effect shown by using the binocular microscope with a deep objective. Fig. *c*, a part of *a*, showing the spaces between the tubes and the mouths of the smaller tubes.

Plate XXXII.—Containing longitudinal sections of *Nematophycus Logani,* Carr. Fig. *a*, showing the irregular direction of the tubes, and a few tubes in the clear space, or "medullary ray." Fig. *b*, a small fragment, showing a series of sub-circular markings on the inner surface of a cell—the "bordered pores" of Dr. Dawson. Fig. *c*, a part of *a*, showing the large longitudinal tubes, and the multitude of smaller tubes matting them together.

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* Vol. xv., p. 477.

space which represents them in a tangential slice shows that they must have consisted of several rows of cells. The rings of growth are rather more than one-tenth of an inch in thickness, and they indicate, in a specimen 1 foot 5 inches in its greatest diameter, and 11 inches in its least, an age of about one hundred and fifty years.

In a subsequent communication to the Geological Society,* Dr. Dawson describes some fossils collected by a member of the Canadian Survey at Gaspé, and among them appears this same fossil under the name of *Nematoxylon crassum*, gen. et spec. nov., with the following description:—"Fragments of wood with a smooth thin bark, and a tissue wholly composed of elongated cylindrical cells with irregular pores or markings. No pith, medullary rays, or rings of growth." With it are associated, in the same genus, some small stems named *N. tenue*, founded obviously upon fragments of wood, but which have nothing whatever to do with it, as Dr. Dawson's figures clearly show. As Dr. Dawson has never given characters to any of the genera which he has proposed in his numerous papers, it would not be possible to determine what consideration induced him to place two structures so different under the same genus, but in this case he tells us he was influenced by the novel consideration that "they correspond in their negative characters"! Such beautiful simplicity in the characters which unite species into genera ought greatly to simplify our systematic works; indeed, I cannot understand on what principle Dr. Dawson, after establishing *Nematoxylon*, did not include all the fossils he has described in this remarkable genus. It certainly swallowed up *Prototaxites*. It is true Dr. Dawson points out four characters in which *N. crassum* differs from *Prototaxites*. These are:—1st. The cells are one-third greater in diameter,—but in the drawings representing the two fossils, in his recently-published 'Pre-carboniferous Plants,' each being magnified to the same extent, the cells of *N. crassum* are the smaller of the two. This is obvious from an examination of the facsimile of Dr. Dawson's figure of *N. crassum* here given (Fig. 1 *a*), as compared with a small portion of his figure of *Prototaxites*, also given in facsimile (Fig. 1 *b*). 2nd. The cells are destitute of the peculiar markings of *Prototaxites*,—but the cells of *Prototaxites* are also always destitute of the markings which Dr. Dawson describes as "peculiar" to them, as will be apparent from the description which follows. 3rd. There are no medullary rays—neither are there any in *Prototaxites*. And 4th. There are no rings of growth,—and this might have been expected, as the largest specimen was only half an inch thick. There seems to be something more in the character given in the general description,

FIG. 1.

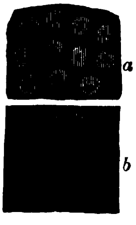

Fig. 1 *a*, copied from fig. 137 *a*, and Fig. 1 *b* from fig. 20 of Dawson's 'Pre-carboniferous Plants.'

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* 1863, vol. xix., p. 446.

that the cells are "much thickened by a ligneous deposit;" but any one accustomed to the investigation of fossilized tissues will, on a hasty glance at Dr. Dawson's figures, see that he has mistaken the nearly complete filling up of the cell-cavity with mineral matter for "ligneous deposit." This condition of fossilization is very common; but the close affinity, if not identity, of *N. crassum* with *Prototaxites* is almost admitted in the 'Pre-carboniferous Plants,' where it is said:—"I place these plants here simply because of the resemblance of their tissues to those of *Prototaxites*, with which it is possible that they may have had some connection, being perhaps stems or slender roots of similar species of smaller size" (p. 20).

When Dr. Dawson visited England in 1870 I had an opportunity of examining, in his company, his specimens of *Prototaxites*. I then pointed out to him some peculiarities in its structure inconsistent with his descriptions. He presented some of his very interesting fossils to the British Museum, and among the specimens of *Prototaxites* was one fragment which he specially pointed out as suitable for microscopic preparation and examination. The drawings accompanying the present paper have been accurately made by Mr. Blair from this specimen.

In a paper "On the History and Affinities of the British *Coniferæ*," which I submitted to the Geological Section of the British Association at Liverpool (1870), I had, in dealing with the *Taxineæ*, to consider *Prototaxites*, which Dr. Dawson informed me he had found in England. A careful examination of the prepared specimens had convinced me that Dr. Dawson's plant was no conifer, and reasons which I shall presently state at length satisfied me that it was an Alga. This opinion I expressed as a reason for my eliminating *Prototaxites* from the *Taxineæ*, as is shown in the only abstract of my paper published at the time, from which I make the following extract:—"The *Taxineæ*, containing nearly 100 living species found all over the world, and represented in Britain by the yew, made their appearance in the Carboniferous rocks, as determined by a fruit described by Dr. Hooker, and shown by him to be nearly related to the living *Salisburia*. The supposed Taxineous wood from the North American Devonians, to which Principal Dawson gave the name of *Prototaxites*, was a remarkable Alga of enormous size. Several Taxineous fruits have been found in the Eocene strata at Sheppey."*

The reporter of another scientific journal, impressed apparently with the notion that this was the one point of interest in my communication, gave the following paragraph as his only account of it:—

"*Colossal Fossil Sea-weed.*—From the microscopic examination of the structure of specimens of the fossil trunks described under the name of *Prototaxites Logani*, and which Principal Dawson affirmed, in his Bakerian lecture before the Royal Society, to be the oldest known instance of coniferous wood, Mr. Carruthers has discovered that they are really the stems of huge Algæ, belonging to at least more than one genus. They are very gigantic when contrasted with the ordinary Algæ of our existing seas, nevertheless some approach to them in size is made in the huge and tree-like Lessonias which Dr. Hooker found in the Antarctic seas, and which have stems about 20 feet high, and with a diameter so great that they have been collected by mariners in these regions for fuel under the belief that they were drift-wood. They are as thick as a man's thigh."*

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* 'Nature,' Oct. 6, 1870, p. 464.

This paragraph came under the notice of Dr. Dawson in a reprint in the 'American Naturalist' for May, 1871, and he met it with the following reply:—

"*The earliest known Coniferous Tree.*—Allow me to say, with reference to a notice copied in your May number from the 'Academy,' that the opinion respecting the plant above named, attributed to Mr. Carruthers, is an entire mistake. *Prototaxites Logani* is an exogenous tree, with bark, rings of growth, medullary rays, and well-developed, though peculiar woody tissue; and if Mr. Carruthers has made such a blunder as that attributed to him, this can only be excused by defective observation or imperfect specimens.—J. W. Dawson."†

Unwilling to continue a discussion after this fashion, and aware that Dr. Dawson proposed publishing a further account of the Devonian plants of Canada, I addressed to him a letter, containing at length my interpretation of the structure and affinities of this fossil, with the view of preventing the republication of obvious error. I regret that my effort was futile. In 1871 Dr. Dawson published, as a Report to the Director of the Geological Survey of Canada, his work on 'The Fossil Plants of the Devonian and Upper Silurian Formations of Canada.'‡ All the errors of observation which I had pointed out to Dr. Dawson in his own specimens when he was in London, and shown in detail by drawings and descriptions in the letter to which I have referred, are here repeated and expounded, and the same absurd generalizations as to affinities, &c., are indulged in. My communication is made the subject of the following note on page 19, and this is the only use made of it:—"Mr. Carruthers has kindly pointed out to me some structural points in which this remarkable plant resembles Algæ of the family *Codieæ*, the long tubes traversing which he compares with the cells of *Prototaxites*. For the reasons stated in the text, however, I cannot accept this as an indication of true affinity, and must believe the plant to have been a terrestrial tree exogenous in its mode of growth. The high botanical skill of Mr. Carruthers, however, renders it important to state his views in the present imperfect state of our knowledge of this truly wonderful plant."

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* 'Academy,' Oct. 1870, p. 16.

† 'American Naturalist,' vol. v., p. 245.

‡ Quoted in this paper under the title 'Pre-carboniferous Plants.'

I will recur to this note again, and will only now stop to say that nothing of any importance is added in this last publication, as is evident from the specific characters, which it will be noticed scarcely differ from those which were given in the original paper; and where they differ it is only to commit Dr. Dawson more completely to error. The diagnosis is as follows:—"Woody and branching trunks, with concentric rings of growth and medullary rays. Cells of pleurenchyma not in regular lines, cylindrical, thick-walled, with a double series of spiral fibres. Disks or bordered pores few, circular, and indistinct. The specimens are usually silicified, and the bark in a coaly state."*

It is necessary to say that my descriptions are made from a specimen given me by Dr. Dawson, for the purpose of having it prepared for microscopical inspection. It is in the condition which he thus describes:—"The wood appears as a homogeneous black cherty mass, only faintly marked with a longitudinal striation parallel to the fibres. It presents its structures in a perfection unsurpassed by any fossil wood known to me."

The large and lighter-coloured fossils referred to by Dr. Dawson, and specimens of which he presented to the British Museum, have also been sliced. In one of them the tissue is entirely made up of spherical cells; and this appearance is not due to concretionary action, but to the limits of the cells, the walls of which can still be detected. In other specimens the spherical structure is due, as Dr. Dawson says, to concretionary action. The largest of these specimens contained an irregular-shaped cavity, which was filled with minute free double-headed prisms of quartz. I shall, however, dismiss these specimens from consideration, and confine my attention to that on which Principal Dawson's histological investigation of the plant is based.

Under a low power a transverse section exhibits a somewhat loosely aggregated mass of circular openings of nearly uniform size, except that there are recurring tracts of smaller and more closely aggregated openings. These tracts have a concentric arrangement, and are the rings of growth of Dr. Dawson (Plate XXXI., Fig. *a*). On submitting the specimen to a higher power, we find that the circular openings have well-defined walls of considerable thickness, and that they are nearly uniform in size, except where the tracts of small diameters occur to which I have referred (Plate XXXI., Fig. *c*). It is obvious that all these openings represent the same kind of tissue, that is, as far as they are concerned, the structure is perfectly uniform and simple. We further observe that, while the majority are cut transversely, some are cut more or less obliquely, showing

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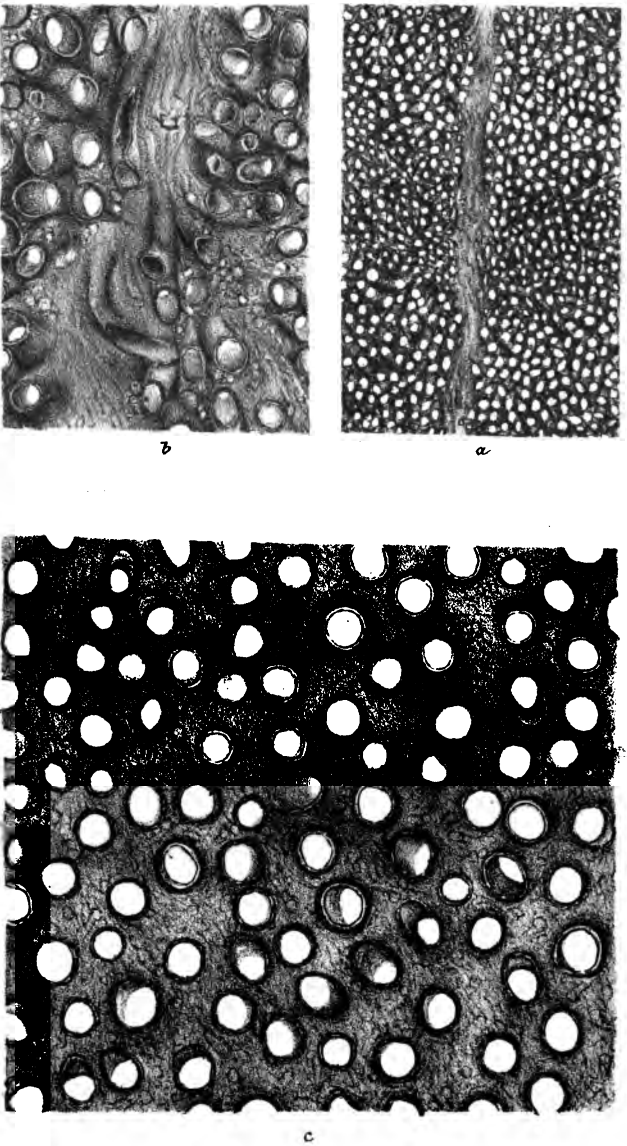
* 'Pre-carboniferous Plants,' p. 16.

The Monthly Microscopical Journal, Oct. 1. 1872. Pl. XXXI.



D. Blair, ad nat. lith. W. West & Co. imp.

Nematophycus Logani, Carr. Prototaxites Logani, Dawson.
From the Devonian rocks of North-America.

that the direction of all of them is not truly at right angles to the section. Thus, they are separated from each other by spaces often as great as the width of the opening, and sometimes much greater. The merest tyro in histological botany will see at once that we have gone sufficiently far in our investigation of this stem to show us that it cannot belong to a Phænogamous plant. It wants the elements of the definite fibro-vascular bundles of the monocotyledon, and it wants also the elements of the indefinite fibro-vascular bundles of the angiospermous dicotyledon. Nor can it have anything whatever to do with the woody tissue of the gymnosperms, for the wood cells of the different bundles of these plants are universally and necessarily in close approximation, even in "the young succulent twigs of some modern conifers," in which Principal Dawson erroneously says the wood cells are loosely aggregated in relation to each other.*

The space between the large openings is not uniformly structureless, but scattered throughout it can easily be detected openings of smaller size. The walls of these small openings are very delicate, and their interior is filled with mineral substance of the same colour and condition as that which invests them. In this respect they differ from the larger openings, which are filled with a less coloured and more transparent condition of the mineral.

It is important to appreciate fully the mineral condition of this plant. It is entirely converted into amorphous silex, so that not only are the tissues replaced by this substance, but the spaces between the tissues and the cavities inside the tissues are completely filled with it. It is my purpose to deal one day with the different conditions under which fossil plants are preserved. It is too large a subject to treat of by the way. I would only say here that the condition of *Nematophycus* is not uncommon, and that most beautiful specimens of the same condition occur in the silicified plants from Antigua. In the Brownian collection in the British Museum there is a specimen of the terminal bud of a palm, in which all the tissues are replaced with silica; the interior of the cells and vessels are filled with this mineral, and the spaces between the leaves are occupied with the same substance which, from its colour and condition, was evidently supplied at the same time that the organized tissues were being fossilized.

Occasionally spaces more or less free from organized tissues are seen passing through the structure in a somewhat radial direction (Plate XXXI., Fig. *a*). These are the medullary rays of Dr. Dawson. It must, however, be noticed that the direction, though in a general way radial, is not strictly so, but that the spaces have an irregularly tortuous direction. Thus the sides of the spaces are very irregular, coming together here and there, swelling out into irregularly-shaped pouches, and turning off to the right or left. Further, the larger tubes often pass through them from one side to the other (Plate XXXI., Fig. *b*, and Plate XXXII., Fig. *a*); and finally no medullary cellular tissue has ever been detected in them either by Dr. Dawson or myself, although the delicate-walled smaller tubes are well preserved, and may often be seen passing through them.

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* 'Quart. Journ. Geol. Soc.,' vol. xv., p. 484; 'Pre-carb. Plants,' p. 18.

If we now turn to the longitudinal section, we shall find much additional light thrown on the structure of this fossil. Under a low power (Plate XXXII., Fig. *a*), we see that the large tubes are not parallel, but that while their general direction is the same, they pass in and out in an irregular manner. They are, moreover, continuous tubes without any discoverable termination. This is well shown in the singularly faithful drawings by Mr. Blair. I have most carefully again and again searched for the termination of a tube without success, except in one instance, when examining a slide with my friend Dr. Wittrock, the Algologist, I detected a horizontal septum, with the slightest indentation on the periphery of the wall, showing that it was not accidental to the fossilization, but that it had been present in the original structure. Although I marked the slide I have been unable again to detect the particular tube, else I would have had it figured. Here again I am in direct opposition to the statements and drawings of Dr. Dawson. He says, "The wood cells are seen to be much elongated, and to terminate in conical points."[*] And his figure[†] clearly shows this, as well as that (fig. 24) on plate ii. of his 'Pre-carboniferous Plants,' though the text is somewhat modified, but not really corrected. Dr. Dawson has obviously been misled by the way in which one tube is hid by the passage over it of another. I had pointed out this defective observation on his part in my letter, as is evident from the note already quoted. How far this letter led to the modification of the text I cannot tell, but it is remarkable that the text differs from the plates in so important a point.

With a higher power than that employed in Fig. *a*, the true nature of the tubes becomes more apparent. The wide spaces which separate them are as obvious here as in the transverse section, and the character and direction of the smaller tubes are clearly exhibited (Plate XXXII., Fig. *c*). If one may speak of one blunder as more gross than another in Dr. Dawson's description of this fossil, that designation must be given to the error which he has made in connection with these small tubes. In every description published by him he says, "Cells of pleurenchyma with a double series of spiral fibres." This error in observation (persevered in after I had pointed it out to him in his own specimen) suggested the Taxineous affinity of the fossil, and influenced the drawings of the plant. I have already shown that the large tubes cannot be called pleurenchymatous cells, and now I have to show that the "double series of spiral fibres" are not inside the large tubes, but are independent

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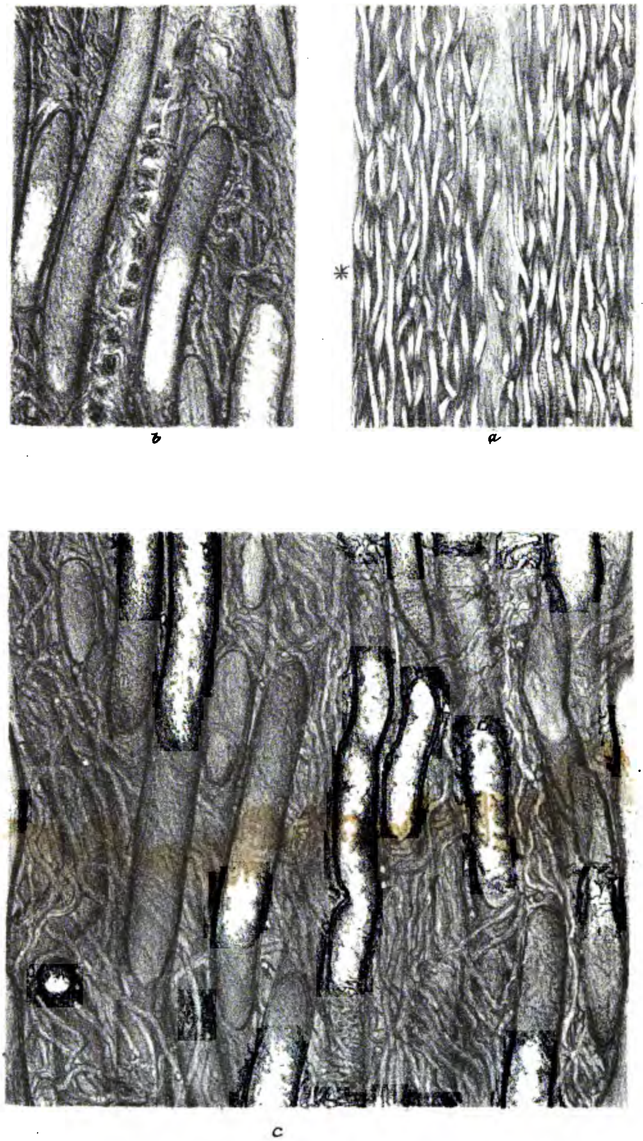
[*] 'Quart. Journ. Geol. Soc.,' vol. xv., p. 485.

[†] *L. c.*, p. 484, figure 5 *b*.



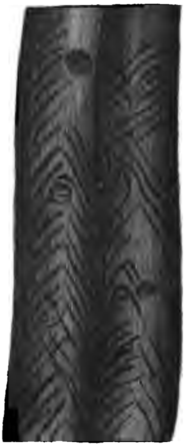
D. Blair, ad nat. lith.
W. West & C^o. imp.

Nematophycus Logani, Carr. Prototaxites Logani, Dawson.
From the Devonian rocks of North-America.

structures external to them! They are indeed the small circular openings seen occupying the spaces between the large tubes in the transverse section, and which in the longitudinal section we find to be slender tubes passing in and out among the large tubes in a more or less oblique direction. An intelligent or educated observer would at a glance see the true nature of these smaller structures, though the great number of them is not at once apparent. This is owing to the delicate nature of their walls, as well as to the fact, which I have already stated, that the fossilizing mineral which replaces their tissues is of the same colour and structure as that which fills the spaces in which they occur. Continued observations with good light and a good objective (the drawings were made with a ⅛th of Ross) will exhibit their great number and their irregular direction, so well expressed by Mr. Blair in Plate XXXII., Figs. *b* and *c*.

That the strong terms in which I am compelled to speak of Principal Dawson's descriptions and drawings may be justified by those who have not access to his memoir, I have copied in facsimile a drawing from plate ii. of his 'Pre-carboniferous Plants,' in which, as in other drawings, the print is made to tell the erroneous observation which Dr. Dawson has given to it. This figure is thus explained:—"*Prototaxites Logani*, three (two) wood cells, showing spiral fibres and obscure pores at *a a*, 300 diameters." It is not a restoration. There is a note at the foot of page 18 of 'Pre-carboniferous Plants' which is in strange contrast to the text. How far it owes its existence to my letter I cannot say, for in it I pressed on Dr. Dawson's notice how completely the existence of these smaller tubes destroyed his whole theory. The note is as follows:—"In some of the more perfect specimens the fibres appear as if connected with each other by fine reticulations, or by the dark bars of the thickened walls passing from one to another. This curious appearance it is difficult to explain. It may either depend on the state of preservation of the specimens, or on some peculiarity of structure at present unknown to me." If Dr. Dawson knew anything whatever about a vegetable cell, and the formation of the spiral fibre in its interior, he would not have written such nonsense as the first sentence in this footnote. A little further reading or investigation would have saved him from the equally fatal blunder of describing coniferous disks or bordered pores on the walls of tubes which are not in contact, but are widely separated from each other. Principal Dawson will find that coniferous disks could not exist in such circumstances. The markings on which the notion is based are not common; but in the slides I have examined I have

FIG. 2.



Copied from Plate II., fig.26, of Dawson's 'Pre-carboniferous Plants.'

seen several, sometimes singly or scattered irregularly on all sides of the walls of the tube, rarely in a continuous series, as in specimen figured at *b* on Plate XXXII. These markings are irregular in outline, and are generally split up into irregular polygons by cracks which pass right through them. They are without doubt placed on the inner surface of the tube. This may be determined by careful focussing, and it is distinctly shown in such a section as that shown on Plate XXXII., Fig. *a*, where a tube is seen opposite the asterisk in longitudinal section, and the markings are on the inner surface of the wall of the tube.

The walls of the large tubes are of considerable thickness. When highly magnified they exhibit a series of minute dots covering their surface. Mr. Archer, in the account of his examination of a section of the fossil, says, "the principal longitudinal tubes appeared on transverse section to have a wall concentrically stratified."* I have most carefully searched for this appearance without being able to detect it. In some cases where the section of the tube is slightly oblique, I have noticed an indication of a concentric arrangement; but this was due, I believe, to the different planes of minute dots coming into view as the object-glass was being moved towards the specimen. I do not, however, believe that so experienced and accurate an observer as Mr. Archer could have been misled in this way: the cautious way in which his opinion is expressed induces me to express the hope that he may again examine the specimen in the light of this criticism.

The description of this fossil which I have given, and which has been of necessity so destructive of what has been said before regarding it, establishes that its tissues are of the kind known to botanists as "tela contexta." They consist of elongated cylindrical cells of two sizes, interwoven irregularly into a kind of felted mass. No other tissue of any kind exists in the structure of the fossil. This peculiar tissue is very limited in its occurrence in the vegetable kingdom as it at present exists, being confined to the three lower groups of the cellular cryptogams. It forms the cottony medullary layer in the foliaceous Lichens, the mycelium of the Fungi, and the substance of some Algæ. No one would venture to suggest any relations with the two first Classes, and though *Nematophycus* is an anomalous Alga, it must be placed in this group of plants. We must remember that we are dealing with a portion of the vegetative structure only; the pseudo-exogenous appearance seems to indicate that that portion is an axis.

The sea-weeds in which a structure corresponding to that in *Nematophycus* occurs, are the *Siphoneæ*, *Dasycladeæ*, and *Valoniaceæ*, orders of Chlorosperms, the class which without doubt occupies the lowest place among the Algæ, because of the simple structure of the plants included in it. The three Orders which I have named have fronds which are composed of one or many filamentous unjointed cells. In *Siphoneæ* the whole plant is composed of one branching cell as in *Bryopsis* and *Caulerpa*, or of many filiform branching cells joined together into a spongy mass, as in *Codium* and *Halimeda*. In the *Dasycladeæ* the simple or branched unjointed stalk gives off whorls of jointed filaments, as in *Acetabularia*. And in *Valoniaceæ* the stalk is also composed of many inarticulate cells united into a firm body by the small horizontal branching tubes, whilst the head is composed of many jointed filaments, as in *Penicillus*. Excluding the jointed filamentous growths of the latter two orders, their structure agrees with that of the *Siphoneæ* in being composed of one or many ramifying simple filamentous cells. And inasmuch as the portions of *Nematophycus* with which we are acquainted are, as I have said, almost certainly portions of a stalk, we can only get a key to its affinities by an investigation of similar parts in existing plants. The articulated frond or stem of *Halimeda* is composed of a series of large tubes (Fig. 3), which give off numerous smaller and repeatedly branching tubes which pass out in a more or less horizontal direction to the exterior of the plant, where they cohere together and form a thin superficial layer of small spherical cells as seen from the outside.

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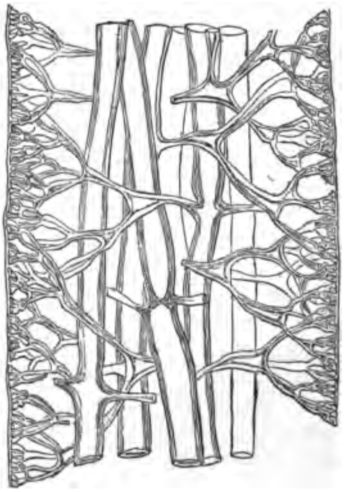
* 'Quart. Journ. Micr. Sc.,' vol. xi., p. 319.

The stipes of *Penicillus* often attains a considerable thickness, yet it is wholly composed of a multitude of densely interwoven longitudinal unicellular filaments, which send off laterally slender branches that repeatedly branch and pass out, as in the lobes of *Halimeda*, to form the periphery of the stem.

FIG. 3.



Halimeda Opuntia, Lam. From Kutzing's 'Phyc. Gen.,' Pl. 43, fig. II., 1.

As far, then, as the presence of large longitudinal unicellular filaments, and of smaller more or less horizontal filaments are concerned, the structure of *Nematophycus* agrees with the stems of these three orders of Chlorosperms. I have not been able to determine the connection of the smaller with the larger tubes in the fossil, as one would expect from the analogy with the recent forms. My friend Mr. James Smith, F.L.S., also made a careful examination of specimens with the view of detecting any relation between the two sets of tubes, but without success. I am further indebted to Mr. Smith for confirming my other observations on *Nematophycus*. His great skill in microscopic manipulation and observation has been of service to me in this, as in many other investigations. It must be remembered, however, that in this fossil, we are dealing with only a fragment of what must have been a very large organism. Perhaps the size may be thought to militate against the affinity of the fossil with the recent Algæ. As I have before said, some of the specimens included under the names *Prototaxites Logani* are not even congeneric with the one which has been figured, and I believe those which I have excluded are the large stems on which Dr. Dawson's statements of the great size of *Nematophycus* are based. No doubt, however, the fragment exhibiting the structure described, belonged to a plant of considerable size. But mere bulk is of little importance among Algæ. Their increase depends simply on the repetition of similar vegetative parts, which in many recent species goes on to almost any extent.

It may be mentioned that among the living Algæ to which I have been referring, there are some remarkable phenomena in regard to vegetative bulk, if not of the organism as a whole, at any rate of the component parts. Take, for example, the extraordinary size of the single branching cell which forms the frond of the different species of *Bryopsis*, which is often several inches in length; or the saccate cells of *Valonia*, sometimes as large as a walnut, and often as large as a hazel-nut; or still more wonderful, the frond of *Caulerpa*, sometimes 2 feet in length, and yet composed of a single cell! The mere repetition of the vegetative parts, until they attain great dimensions, is not so remarkable as these. However, among existing Algæ are to be found some of the greatest masses of vegetable organisms. In the Northern Pacific a species of the genus *Nereocystis* attains a length of 300 feet, and spreads out on the surface of the water a great mass of green fronds, supported on a large floating bladder 6 or 7 feet long. Yet notwithstanding its enormous size, it appears to be only of annual growth. Much larger than this is the *Macrocystis pyrifera*, which Dr. Hooker, on reasonable grounds, has estimated to attain a length of 700 feet. And, it may be as regards external form, we have in the arborescent Lessonias a nearer approach to these Devonian Algæ. These Algæ attain a height of 25 or 30 feet, and their stems, from their exogenous growth, are sometimes a foot in diameter.

The pseudo-exogenous growth is well known in different Algæ. Reference has already been made to the giant Lessonias of Antarctic seas, but I might adduce the Laminarias of our own shores in which this exogenous growth also occurs. In mentioning these two genera, both belonging to the same group (*Laminariaceæ*) of the Melanosperms, and both characterized by the possession of pseudo-exogenous stems, it is worth adding, by way of caution in generalizing, that the method of growth is different in the two genera. In

Laminaria the exogenous increase is from below upwards, according to the growth of the roots, while in *Lessonia* the growth is from above downwards, in proportion to the increase of the leaves.*

A reference now to Dr. Dawson's specific diagnosis will show that in every particular save one it is erroneous. The fossil does not possess a *woody* trunk, and it is not penetrated by medullary rays. The cells are not pleurenchymatous, and there is no double series of spiral fibres. There are no disks or bordered pores. One character only remains, that the trunks have concentric rings of growth; but this is again and again erroneously employed to establish the exogenous, that is, the phænogamous position of the fossil.

It is difficult to conceive how such a series of errors first took possession of Dr. Dawson's mind, and it is still more difficult to realize how that, after the errors were pointed out, first in his own preparations and then in a private and friendly letter, they were persisted in by a searcher after truth. The characters, which are apparent at the first examination of a prepared section of *Nematophycus*, which are obvious at a glance in Mr. Blair's drawing, and which can be to some extent seen even in Dr. Dawson's faulty drawings, especially in the transverse sections, are sufficient to convince any one who has an intelligent acquaintance with the elements of histological botany. It is certain that from this botanical point of view I have no ground in common with Dr. Dawson. Had it been so, there never would have been occasion for this communication.

I have said that *Nematophycus* is an anomalous Alga, and, indeed, that with the materials known it is not possible to correlate it with certainty with any known group of Algæ. Since I first directed my attention to this remarkable fossil, I have lost no opportunity of submitting the preparations to botanists who were acquainted with histology, and especially those who had devoted their attention to Algæ. It is superfluous to say that I have never met with one who for a moment imagined the fossil could be coniferous. On the contrary, the universal testimony has been that it is an Alga.

Archer, of Dublin, thus describes the fossil in his Reports of the Proceedings of the Dublin Microscopical Club:—"There was no appearance of 'disks'; both longitudinal and vertical sections seemed to indicate that the mass was composed of a number of tubes running in a nearly parallel direction, rarely bifurcating, and seemingly not septate or tapering, and with an intercellular medium, apparently formed of minor tubes. . . . . The appearance of medullary rays was probably produced by accidental cracks or fissures, no structure corresponding to them being shown under the microscope. The 'lines of growth' would have their parallel in the pseudo-exogenous stems of the existing *Lessonia*. In connection with this Alga, it is a curious coincidence that Dr. Hooker remarks that in the Falkland Isles the ignorant observer at once takes the trunks of *Lessonia* washed up on the shore for pieces of drift-wood."*

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* See Berkeley's 'Introduction to Cryptogamic Botany,' p. 57.

Prof. Dickie, of Aberdeen, whose algological labours are well known, says, "The plant must have been an Alga; but unless we know the structure from centre to circumference, its exact place cannot with certainty be determined."

My friend Mr. Lindahl, of Lund, was so good as to submit a slide to Agardh, and he sends me the estimate that Agardh formed of it in the following words:—"He says that it is not like the texture of any known species of Alga, but still it is far less like anything else that he knows, and that he cannot at all believe that it is a conifer."

It is unnecessary for me to add to these testimonies; I am satisfied to leave the decision in the hands of those who know anything of histological botany. The faithful drawings that accompany this paper supply sufficient data for appreciating the structure, and for forming a proximate estimate of the position of the fossil. This is not a matter for the mere geologist.

The following short exhibition of the systematic position of the fossil, with the diagnosis of the genus, and the synonymy, will fittingly close the paper.

ALGÆ.

Class.—CHLOROSPERMEÆ.

Inter *Siphonearum*, *Dasycladearum* et *Valoniacearum* Ordines.

NEMATOPHYCUS. Gen. nov. Stem wholly composed of innumerable longitudinal unjointed cellular filaments woven together by a multitude of small sub-horizontal cellular inarticulate tubes, so as to form in the living plant a compact spongy mass.

Sp.—*N. Logani*, Carr.

Syn.—*Prototaxites Logani*, Dawson, 'Quart. Journ. Geol. Soc.,' vol. xv. (1859), p. 484, Fig. 4. 'Pre-carboniferous Plants' (1871), p. 16, Plate II.

Nematoxylon crassum, Dawson, 'Quart. Journ. Geol. Soc.,' vol. xix., p. 466, Plate XIX., Fig. 24. 'Pre-carboniferous Plants' (1871), p. 20, Plate XI., Fig. 137.

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* 'Quart. Journ. Microsc. Science.' New Series. Vol. xi., pp. 319, 320.

IV.—*On the Active Part of the Nerve Fibre, and on the probable Nature of the Nerve Current.* By Lionel S. Beale, Fellow of the Royal Society and of the Royal College of Physicians, Physician to King's College Hospital.

Most observers will agree in the opinion that the particular portion of the nerve fibre concerned in the transmission of nervous impressions is in all probability the axis cylinder. This view will be generally admitted, at least as regards those extended nerve cords, which in man and the higher animals intervene between the central origin and the peripheral ramifications of nerves. This intermediate portion of the nerve cord varies much in length in different cases, and it may be regarded as *non essential,* because in all animals we have parts of the nervous system which are active in which this portion of the nerve does not exist at all; while in some animals, the part of the nerve fibre under consideration is altogether absent, and in none is it to be found at a very early period of life, when nerve action is nevertheless certainly taking place. This intermediate portion of the conducting cord then may vary wonderfully in length, like an electric wire, without any great difference in the action resulting. In the smallest mammal it cannot be more than half an inch in length, while in the whale it extends to many feet. Nevertheless, impressions pass from one end to the other with great rapidity in both instances.

Now the active part of the nerve fibre distributed to the peripheral nerve organ which receives the impressions, exhibits the same general structure and anatomical arrangement in all cases. It is invariably a pale, very transparent, faintly granular, but in the natural state perfectly invisible cord, composed of still finer fibres, or consisting of a soft material which is traversed by currents which take several different lines or paths along it, as may be distinctly demonstrated. This has been regarded as an extension of the axis cylinder, and in an anatomical sense the conclusion is nearly true. The statement is not, however, perfectly accurate, because in many specimens it is evident that the so-called medullary sheath of the nerve and the axis cylinder gradually become altered until the delicate terminal ramifications of the nerve fibres are reached. The fibres of this terminal portion of the nerve network *never resemble in structure the central part of the extended cord already referred to.* Indeed, in the adult mammal these two parts of the nervous system exhibit such remarkable contrast in minute structure, and in chemical composition and physical characters, that it is difficult to believe they are of the same nature. Whatever may be the real nature of the change in the part of the cord midway between centre and periphery, the changes in the more delicate peripheral ramifications are probably far more subtle, succeed one another with much greater rapidity, and are excited with far greater facility.

In this communication I propose to limit myself principally to the consideration of the structure and action of the intervening part of the nerve cord, which is known as the axis cylinder, and is surrounded by and imbedded in a considerable thickness of the so-called white substance of Schwann, or medullary sheath of the nerve fibre. The latter material, which is ten or more times the diameter of the axis cylinder, consists of a fatty albuminous substance known as myelin or protagon. It forms a tolerably uniform layer round the core, which it probably insulates and protects, as the gutta-percha of the electric cable insulates and protects the copper wire which transmits the electric current, and thus prevents damage and loss of conducting property by the contact of adjacent objects. Not only so, but the constitution of the white substance of Schwann or medullary sheath is such as to interfere with the free passage of fluid. It is not very permeable to aqueous or albuminous solutions, and it is probable that by this investment the access of an undue proportion of fluid is prevented, and a uniform degree of moisture in the axis cylinder is preserved. Upon this thin thread-like impression-transmitting axis cylinder all our interest centres. We desire to ascertain how it acts, and we want to know by what means it conducts or transmits impressions direct from centre to periphery, or in the opposite direction. These questions have been often asked, but never conclusively answered. Indeed, in the present state of our knowledge, a conclusive answer is probably not to be given; but it will not be an unprofitable waste of time to consider to what conclusions upon this question we are led by contemplating the facts revealed by careful microscopical investigation.

There is hardly a tissue in the body concerning the structure and action of which we do not possess more positive information than we have been able to gain as regards the exact structure and mode of action of the axis cylinder. A strange mysteriousness still hangs over the question of structure of the axis cylinder. Even the ultra-physicists and chemists, who assert that *all* the phenomena of living beings are physical and chemical, affirm little of a very positive character upon the mode of action of the axis cylinder. Confident as they are in expressing themselves upon most physiological and many medical questions, there seems to be some doubt—even a little diffidence—just the slightest hesitation—concerning the manner in which this little cord carries to us intelligence from the outside world, and brings us into relation with the many objects that surround us. Its arrangement, they admit, looks very like that of an insulated wire; but then it is such a bad conductor of electricity that the nerve force can hardly be of this nature. If it were electricity, surely the conductor *ought to be* metallic! The nerve force, therefore, they conclude, is not electricity. Having affirmed this, all further doubt and hesitation vanish, and we are confidently assured that nerve force is unquestionably some *other mode of force* not yet discovered, but undoubtedly correlated with heat, light, electricity, motion, and the other modes of energy. This, according to the teaching of the present day, is certain. Nerve force must be a mode of ordinary force, because there is nothing else that it can be. And yet one has a misgiving that the advocates of positive-fact knowledge may be exhibiting some little weakness and inconsistency in trusting to this sort of argument. For upon what science principles do they even dare to talk about *new modes* of force they know nothing about? How can they bring the observation and experiment in which only they believe, to bear upon a new mode of force which they admit exists at present only in their own imaginations? Of course we can imagine many things which we cannot investigate; but imagination is not observation and experiment, and imagination has no place in the positive science of our day. New modes of force which *may* exist are but philosophic phantoms—mere fictions of the imagination and of the inventive faculty. Is it reasonable to teach the existence of undiscovered forces correlated with ordinary force? It has been affirmed to be unreasonable, fanciful, frivolous, absurd, &c., &c., to suggest that of undiscovered forces, some there may be which are distinct altogether from ordinary forces, and not correlated with these.

Some consider that the axis cylinder is a soft pulpy material, the relative position of whose ultimate particles is much altered during its active state. Others look upon it as a peculiarly vital form of tissue, although it behaves towards reagents very much as certain forms of fibrous tissue. It certainly is not allied to any form of bioplasm or living matter. Generally the tissue of the axis cylinder has been regarded as very peculiar, and it has been inferred that its peculiar function must be in some manner determined by its peculiar structure or its peculiar chemical composition. Not that there are any new *facts* in favour of this notion; but it has been laid down most authoritatively that the properties of this particular tissue, as of everything else in nature, are due to the properties of the material of which it is composed, and of course to the properties of the elements entering into its composition.

Now when we come to examine this mysterious force-conveying axis cylinder, what do we find? A filament possessing an exceedingly simple structure, which, at least in many instances, looks very like ordinary fibrous tissue. Indeed, if we were shown only a very small piece of an axis cylinder of a frog's nerve fibre, and some pieces of fibrous tissue of the same shape and size, we should not be able with certainty to distinguish one from the other. Can the axis cylinder be regarded as anything more than a very elongated band composed of a texture closely allied to white fibrous tissue, but formed of perfectly parallel and continuous strata, not disposed as distinct fibres, but nevertheless tearing in the longitudinal direction? In many instances, however, the axis cylinder exhibits a tendency to break transversely, and in several of my specimens I have seen oblique lines, but whether these should be regarded as natural I am not sure. In one preparation which has been kept for some time the nerve (axis cylinder) near an oval nerve cell exhibits a distinctly spiral or corkscrew-like arrangement, as if it had been twisted round and round in the same direction during its formation. At the same time I have never been able to demonstrate any remarkable peculiarities of structure in the axis cylinder. Even under the highest powers only a fibrous appearance can be discerned. Anatomical observation almost justifies the idea that if it were possible to replace an axis cylinder by a long filament of ordinary fibrous tissue, we should find that this would conduct the nerve current as effectually as the ordinary axis cylinder itself does. To me it appears very doubtful if the axis cylinder is capable of undergoing any remarkable change in ultimate arrangement during nerve action, and I think that whatever change does take place it is of such a nature that it might occur in other forms of tissue. The peculiarity of the nervous system upon which all its characteristic phenomena depend is probably not any remarkable arrangement of its ultimate molecular structure, but simply uninterrupted continuity of conducting tissue. Nor have we reason to think that the bioplasm which forms nerve tissue grows or lives very differently from other forms of bioplasm. It receives its nutrient material from the same blood and is derived from the same masses which give origin to other forms. Considering the characters and arrangement of the bioplasm matter and its relation to the formed material in all tissues, it is not unreasonable to conclude that currents, and perhaps of the same nature as those discharged by nerve organs, are set free, but that it is only in the case of nerve that an arrangement exists suitable for insulating the currents and for rendering evident, variations in their intensity, rate of transmission, &c.

The general arrangement of the fibres and cells in all central and peripheral nerve organs; the structure and arrangement with respect to one another of all nerve fibres which pass through a considerable distance before they reach their destination; the manner in which nerves act upon contractile tissues, and the circumstance that a current of electricity produces a similar action; and the fact that electricity is set free in special organs which are very rich in nerves, but which do not differ in any essential particulars in ultimate arrangement from other nerve organs in which electricity is not set free, render it probable, as it seems to me, that the current transmitted by the axis cylinder is ordinary electricity, and that all the effects produced upon other tissues depend upon the transmission through nerve fibres of currents of electricity varying in intensity.

But what determines the particular course the electrical current shall take,—which fibres shall be traversed by a strong current and which by a weak one,—how the intensity of the current varies at different times, and is momentarily increased or reduced by the influence of the will,—are questions quite apart from the consideration of the nature of the current itself. But if we can determine the probable nature of the nerve current, or even show that there is great probability of it being determined, one very important step towards the solution of much higher physiological problems will be gained.

Of the Nerve Current.

The general opinion, however, of physiologists, as already stated, seems to be that the nerve current is some mode of force, perhaps correlated with heat, electricity, &c., but not exactly identical with any form or mode of energy known or of which we have as yet any experience. The arguments upon which this view is founded appear, however, to me very inconclusive. Much of the evidence we possess unquestionably favours the view that the nerve current *is* electricity, for many, indeed most, of the phenomena familiar to us may be explained upon this view. Lastly, some physiologists have sought to account for the wonderful phenomena of the nervous system by supposing that some force or power of a peculiar and exceptional kind is at work in nerve systems only.

I shall endeavour to show that if electricity could be made to travel in different directions, and the currents combined in various ways and made to traverse series of conducting cords very specially arranged, according to design, the phenomena of nervous action might be accounted for without resorting to the hypothesis of the existence of a peculiar power, or of some new mode or form of force not yet discovered.* It is at least not improbable that the varying effects noticed in connection with the nervous system may be determined by alterations in the intensity of the current, and in the conducting properties of the fibres, instead of being due to the transmission of *different kinds of nerve force.* One would have thought that it would be more in accordance with the doctrines of physical science to endeavour to explain the phenomena by the action of forces we know something about, than to attribute them to the influence of other forms or modes of force which are purely fanciful and fictitious. At any rate it will be time to call in the aid of such airy nothings when all attempts to explain the facts by known forces shall have failed.

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* Physicists and chemists see no difficulty whatever in assuming the existence of many modes of force of which they can form no conception, and think it very satisfactory to refer phenomena which they cannot understand to some at present undiscovered form or mode of ordinary motion; but if anyone attributes these same phenomena to the influence of some equally undiscovered form of force having no connection whatever with primary energy or motion, he is ridiculed, because, say the physicists and chemists, "there is but one force in kosmos"!

But it is interesting to notice how often minds of the most rigidly physical tendencies seize upon purely conjectural hypotheses, and use them as if they were established truths. It has been surmised that nerve action depends upon a chemical change which is supposed to take place in every part of the nerve fibre. Mr. Herbert Spencer settles the question in the most summary way by boldly asserting that the axis cylinder of a nerve consists of some colloid "matter isomerically transformed with ease." He accounts for nerve action by suggesting that the protein substance of the axis cylinder "is habitually changed from one of its isomeric states to another," while he says that the matter of the nerve cell is the seat "of destructive molecular changes and disengagement of motion."

Chemical Theory of the Nerve Current.

A chemical theory was long held concerning the nature of muscular action, but it was at last admitted, as was, indeed, apparent from the very first, that muscles would have to be destroyed and reformed at a far more rapid rate than it was on other grounds reasonable to suppose possible, if the great amount of energy manifested during their action was really due to chemical decomposition of the tissue of the muscle itself. There was, in fact, no evidence whatever, except that which was distilled from the imagination of the chemist, for the conclusion that muscular tissue did undergo rapid disintegration and reconstruction. From my own investigations of muscular fibres in various animals, I felt quite sure at the very time when these chemical doctrines were in high favour, that the conclusions were thoroughly erroneous. From the study of muscular tissue at different periods of development, and the consideration of various circumstances connected with the growth of muscle and its relation to other textures in a variety of animals, particularly in the class of insects, I was convinced that muscle was a *slowly growing* tissue, and that the work it performed was certainly not due to the chemical decomposition of its material particles. Nevertheless the fact of the change in the reaction of muscle from alkaline to acid is still urged in favour of the doctrine, and some have affirmed that a similar change occurs in nerve. In spite of the statements of Liebreich, Heidenhain, and other observers to the direct contrary, the view that nerve energy is stored up in chemical compounds which undergo chemical change during nerve action is still taught. That such an idea should be stated at all betrays ignorance of the character of the axis cylinder of the nerve itself. If we examine the axis cylinder, say, of the sciatic nerve of a frog, what do we find?—a firm, tough, fibrous-like, flattened band, not easily torn, and evidently consisting of a tissue of slow growth; in fact, the very last characters we should expect to find in a tissue prone to rapid chemical change. Neither is a structure surrounded by ten times its thickness of oily matter (myelin) favourably situated for taking up new materials and quickly getting rid of products of decay. One of the least permeable substances in the body is the myelin of the nerve fibre, and yet through this must pass all the materials from the blood to renovate the disintegrated axis cylinder, if nerve action is due to chemical change in the nerve fibre itself.

The Vibratory Theory of Nerve Action.

Some think that nerve action depends upon the molecules of nerve fibres being thrown into vibration, and the axis cylinder of a nerve has been spoken of as if it had been proved to consist of a chain or chains of very minute spherical particles. But the axis cylinder is not composed of matter which would more readily propagate motor impulses than the matter of ordinary fibrous tissue. Moreover, it varies much in character and in thickness in different parts of its course. The impulses supposed would be much modified during their transmission. This theory leaves one of the most important facts connected with nerve fibres unexplained; for upon such a supposition what purpose could be served by the very thick layer of the white substance of Schwann, and in that part of the nerve only which lies between its central and peripheral distribution? Why do we find, moreover, that this investment is invariably so much thicker where a number of nerves run for a long distance parallel to one another than where they cross one another at considerable angles?

Experimental Investigation Inconclusive.

Most of those who have taken up the subject of nerve action from the experimental side, appear to have had a very imperfect acquaintance with the structure of the tissue upon which they were experimenting. The transmission of electric currents through the nerves of a recently-killed animal, is a very rough operation, and indeed very different in many particulars from the transmission of the natural currents, whatever may be their nature, along the axis cylinder, while it remains in connection with its central cells during the life of the animal.

If we were to take a bundle of several marine cables, and smear the gutta-percha investment carelessly over the cut ends of the copper wire cores, at a distant part of the circuit, and then transmit a most powerful current through the deranged wires, we should not find the needles acting as they did when very delicate currents were made to traverse an individual wire. The differences observed might induce some to conclude that the current by which the instrument was influenced in the normal state, was totally distinct in its nature from that much more powerful current which gave rise to the much greater but irregular and unmeaning disturbances. This reasoning is applicable to the experiments in which strong electric currents have been transmitted along compound damaged nerve trunks.

Fallacy of the Argument based upon the Excitability of Nerve Fibres.

It has been said that the fact that nerve loses its excitability without losing its power of conducting electricity, is a fatal objection to the doctrine that the nerve current transmitted during life is of the nature of electricity. But many things seem to have been entirely overlooked by those who urge this argument with so much confidence. Is it not obvious that soon after death the bioplasm, which is instrumental during its life in maintaining an equable flow of nutrient fluid through the tissue adjacent to it, must cease to effect this important change? Is it not certain that in consequence the axis cylinder of a nerve fibre must be much changed, and especially at the peripheral distribution of the fibres where they are very delicate and ramify naked upon the muscular tissue? No wonder, then, that the muscles fail to respond to the stimulus as before. This fact is attributed to the nerve having lost its "excitability"; but is it not more probable that the true explanation of the fact is, that in consequence of the change in the constitution of the nerve fibre, resulting from the cessation of the currents of fluid through it consequent upon the death of the bioplasm, it fails to conduct the electrical current as it did when in a state of integrity? So far, therefore, from the above fact being an argument against the idea that nerve force is really electricity, it actually affords support to this view.

The power of causing the muscles to contract when the nerve is irritated is lost sooner if the nerve be irritated than if it be at rest. It is increased by heat and decreased by cold. When the nerve is "irritated," the operation is such as would be certain to alter any structure so delicate as the peripheral ramifications of nerves, and impair or destroy their conducting power. Dr. Rutherford has remarked that the nerves of a weak animal conduct faster than those of a strong one. "The velocity is so great in this case that it may be scarcely measurable," a fact which probably depends upon the circumstance that the conducting tissue is more moist. In weak animals the masses of bioplasm are much more abundant than in strong ones, and the tissues contain a large quantity of fluid. The axis cylinder participates in this change, and in this way the remarkable irritability manifested may be accounted for.

Again, it has been argued that because the irritability of a nerve can be destroyed by the electrical current, nerve cannot be a mere conductor of electricity, and that nerves after death are as good *conductors* of electricity as during life, and that frozen nerves conduct electricity, though they will not transmit nervous energy. These objections are considered by many to be fatal to the idea that nerve force is after all only electricity. But a little consideration on the part of those who argue thus, would have convinced them that the facts above referred to are as easily explained upon the electrical as upon any other hypothesis of nerve action.

Little can be gained from the argument that a bit of nerve that has long been dead conducts electricity as well as a nerve just removed from a living animal; for neither are much better or worse conductors than some other tissues of the body. We must remember that in nature the thing that actually transmits the nerve current is the axis cylinder alone, but that in our experiments we send comparatively strong currents through the white substance of Schwann and axis cylinders indiscriminately. The current deranges the axis cylinders, and of course seriously damages the delicate distal ramifications of the nerve fibres.

No one who has seen and studied the ultimate ramifications of nerve fibres in tissues, will suppose for a moment that anything conclusive will be learnt concerning the action of nerves by sending powerful electrical currents through damaged nerve trunks.

Fallacy of the Argument deduced from the Rate at which the Nerve Current travels.

It has been said that the difference in the rate at which nerve energy and electricity travel, is enough to convince us that these two currents are not of the same nature. But the comparison as it has been instituted is not fair. Electricity as it travels along a *copper wire* has been contrasted with nerve energy (electricity) as it travels along a *moist fibrous cord.* No wonder that the rate at which the nerve current travels along the nerves should be very slow as contrasted with the rate at which electricity traverses a copper wire, but such a fact by no means proves that nerve energy and electricity are very different. The nerve current, it has been proved, traverses not more than from 100 to 300 feet in a second of time, while electricity, it is known, travels at the rate of many thousands of miles in a second. The deduction is, however, defective, and in many ways. I would remark that—

1. A comparison is made between the passage of the nerve current along a moist tissue and the passage of electricity along a copper wire.
2. It must be borne in mind that it is certain that if the nerve current were electricity, it would travel very slowly along such a structure as the axis cylinder.
3. The rate at which a single axis cylinder transmits a current of electricity has not yet been ascertained, but this is exactly the information we need before we can arrive at a correct conclusion on the subject.
4. The rate at which electricity travels through such a moist tissue as the axis cylinder, has to be ascertained and compared with the rate at which nerve energy is known to travel.

I will only remark, in conclusion, that no one has yet succeeded in showing that the nerve current is *not* electricity, while a great number of well-ascertained facts, especially those known in connection with the structure and action of the electrical organs of certain of the lower animals, strongly support this inference.

V.—*On the Regeneration Hypothesis.*

By Dr. Louis Elsberg, of New York.

The more the knowledge of the facts spreads on which Lamarck's Development theory and Darwinism rest, the more the need of a logical conception as to the propagation of organisms becomes felt. Without detailing other attempts in this regard I desire to present a brief account of my hypothesis of regeneration. Its fundamental proposition is the following:—*The germ of every derivative living being contains (resp. consists of) plastidules of its whole ancestry.*

As is well known, Prof. Haeckel has given the name of Plastids* to the four elementary forms of living matter, *viz.* cells with and without investing membrane, and cytodes with and without investing membrane. Adopting the name plastid, and without entering upon the vexed questions connected with the atomic hypothesis, I use the term "plastidule" to designate the smallest conceivable actually existing—the almost infinitely subdivided—particle of the matter composing a plastid.

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* As more conformable with the genius of the English language, I have omitted the final "e" of the German word *Plastide*, and of course in pronunciation place the accent on the first syllable instead of on the second.

It will readily be admitted by everybody that the impregnated ovum from which a child proceeds, consists of matter (of plastidules) derived from the bodies of its parents; in the same manner the germs from which each of these parents was developed existed in the bodies of their parents; and it requires but little reflection on the incontestably very great divisibility of matter to understand that actual material particles of the grandparents may be contained in the germs of the grandchild. If we add to our argument proofs and illustrations of the dependence of function upon matter, and point to the various laws of heredity, especially to the transmission to grandchildren of resemblances and peculiarities of grandparents, the way is clear to the exposition that the germ of a child contains actual particles of matter, *i.e.* plastidules, of all its ancestors. By thus proceeding backward in tracing the genealogy of man, I have more than once succeeded in obtaining the assent of very strong opponents of the theory of evolution to my views of the development of man down to his descent from "Adam and Eve." One advantage of this hypothesis is that its adoption does away with the relevancy of the most usual arguments against Darwinism,* and greatly facilitates the acceptance of the evolution theory by both the lay and scientific public.

I call it the hypothesis of regeneration because according to it the ancestors are to a certain extent bodily, and therefore also in every other respect, born again in their progeny.

Commencing with a parental pair, Adam and Eve, we may assume that their children came from germs which were wholly derived from their bodies; that the germs of the children of these children contained, mixed with the possibly modified plastidules of their immediate progenitors, some of the original plastidules of the first parental pair; and so on in each succeeding generation. To express this hypothesis arithmetically, I might say that in each succeeding generation the numerator remaining the same, the denominator of the fraction of each set of ancestral plastidules increases.

But we do not rest with the assumption of Adam and Eve. Let us suppose that under appropriate circumstances the combination of the requisite elements has given rise to the simplest protists, moneres, "protoplasm." (And in this connection I would in parenthesis say what everybody must admit, *viz.* that *if* all the requisite conditions are present, the conversion of inorganic elements into living matter must necessarily take place. No one who considers the subject seriously doubts that at some past time in the history of our globe such conversion has taken place, the difference among men being that some ascribe it to the exercise of direct creative agency in each individual case, while others ascribe it in the first instance to the agency of properties inherent in matter, and to "evolution into higher forms" subsequently; but, I repeat, the fact of such conversion must be admitted by everyone, whether he be the most ultra freethinker; whether he be the most liberal interpreter of the Bible, which says of man, "And the Lord God formed man of the dust of the ground; and breathed into his nostrils the breath of life, and man became a living soul;" or whether he be anything between these two extremes. It is not necessary for the exposition of my hypothesis to inquire what the requisite conditions of the change of not-living into living matter may be, but as to their presence I have emphasized the "*if*" to avoid the possibility of controversy even with those who believe with Prof. Huxley that they are of such physical and chemical a nature as must have existed at some remote period of the earth's history, but which can no more occur again than that an adult man can pass through all the conditions of his infancy again. For my own part, however, I confess that while I do not know exactly what these conditions are, and admit that human agency has never succeeded in bringing them demonstrably about, I can see no difficulty in believing that they may occur in the laboratory of nature at any time—at the present day as well as in the long past age—and that whenever they occur—at the present day as well as in the remotest age—the production of living protoplasm from inorganic elements takes place.)

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* See *Thesis* 4, at end of article.

A lump of protoplasm subjected to inspection, under appropriate circumstances, can be observed to become larger by taking up, *i. e.* by combination or mixture with similar plasmic matter, and finally to divide. This so-called "spontaneous division" my hypothesis regards as the consequence of the mixture of two little lumps, similar, but not entirely alike. Two specks of protoplasm, no matter how perfectly alike they may be supposed to be at their production, cannot remain identically similar, because their existence and surroundings in space, and therefore the modifying influences to which they are exposed, cannot be identically the same. If we assume that two such specks combine and are thoroughly mixed with each other, and then divide, we may draw the following two inferences:—1, the resulting specks differ from each of the original specks, inasmuch as they contain plastidules of both; and 2, the resulting specks represent accurately the constitution of the two original specks, but not as these were at the time of their production, but as they are at the time of mixture. We thus arrive at the most elementary conception of the two fundamental laws of the phenomena of all living beings, *viz.* adaptation and heredity. I use the general expression "adaptation" like Haeckel, to denote the reaction of all influences which affect the being, *i. e.* all modifications of its constitution which occur from the moment of its production to that of its death. While adaptation involves change, heredity preserves in the progeny the constitution of the parental organisms as it is at the time of mixture. The degree or amount of actual change which has taken place in an organism after a long series of generations is a very complicated resultant, the individual factors of which, science is at present only extremely seldom able to show.

Without prolonging this article by the attempt to follow in detail any of the innumerable generations through which the protoplasm had to pass to be developed into a higher organism, I will simply state that the hypothesis of regeneration seems to me to be in accord with all the hitherto ascertained facts of development, to explain in an unconstrained and natural manner many of them, and to run counter to none.

1. The most highly developed organisms are the oldest existing, those in their constitution nearest to protoplasm the youngest; 2, the furrowing or division of a germ into innumerable parts is caused by mixture of different plastidules having occurred innumerable times; 3, the plastidules to be mixed though differing, must not differ more than to a certain, relatively very small, degree; greater dissimilarity prevents the mixture; 4, the germs of various organisms existing at the present day, though they may appear ever so closely related or alike, are, if they have passed through different stages of development, so completely dissimilar in their actual constitution or mixture of plastidules, that the one organism can never be developed from the germ of the other; 5, the derivation from lower organisms is the cause of the various stages of development through which the germ of higher organisms passes, and on this depends the fact so prominently stated and illustrated by Haeckel that "the ontogeny of every organism repeats in brief time and in great general outlines its phylogeny," *i. e.* the individual development of every organism, or the series of forms through which it passes from germ to completed form, repeats approximately the development of its race, or the series of forms through which its ancestors have passed; 6, that the various organisms now existing form a series, or so to speak a chain of gradations, *more or less* resembling the series of forms through which higher organisms have passed phylogenetically and do pass ontogenetically (embryonically), is due to the greater or less resemblance of the influences to which organisms originally derived from more or less similar protoplasmic matter have been and are subjected.

Finally, the hypothesis of regeneration does not necessarily oppose the hypothesis which Darwin names pangenesis, and advocates so learnedly, in the essential features of which Darwin has been anticipated already by (if my memory serves me correctly) Dr. Benedict Hösch in the beginning of this century.

VI.—*On the Use of Monochromatic Sunlight, as an Aid to High-power Definition.*

By Dr. J. J. Woodward, U. S. Army.

A few years ago I published some brief remarks "On Monochromatic Illumination."[*] These remarks were suggested by the perusal of a letter from Count Francisco Castracane published in the same Journal some time before.[†]

Count Castracane's method consisted essentially in the use of a prism by which the sunlight was decomposed, and any selected colour could be employed, blue or green seeming to him most advantageous. Mine consisted in passing the sunlight through a cell containing a saturated solution of the sulphate of copper in ammonia, which transmits a bluish violet light, admirably suited to high-power definition, and less fatiguing to the eye than any other colour.

At the time I supposed Count Castracane's method to be new; the one I employed I ascribed to Von Baer.[‡] I have since learned that I was in error in both particulars. The proposition to escape chromatic aberration by employing monochromatic illumination goes back in fact to a very remote period in the history of achromatic microscopes; and monochromatic lamps, as well as the use of the prism and of glasses and coloured fluids as absorptive media, were early suggested. It would carry me away from my present purpose to go into a detailed history of the various attempts made from time to time in these directions. As the construction of achromatic objectives continued to improve, these devices fell into obscurity, and it is only of late that attention has been directed to them anew. As for Count Castracane's method, without going farther back, a full account of all the principles involved in the use of the prism for attaining monochromatic light to illuminate the microscope will be found in chapter vii. of the article on the microscope in the eighth edition of the 'Encyclopædia Britannica.'[§]

The use of the solution of the ammonio-sulphate of copper to exclude certain portions of the solar rays, especially for photographic purposes, would appear to have been first suggested by one of our own countrymen more than thirty years ago.

Professor W. J. Draper published in the 'Journal of the Franklin Institute' of Philadelphia, during the year 1837, a series of "Experiments on Solar Light," in the course of which several observations on the properties of the ammonio-sulphate of copper are recorded. In one of these papers[*] he states that the ammonio-sulphate solution absorbs the red and yellow rays of the spectrum, and with them so much of the heat, that but "twenty rays for every hundred that fell upon it" were transmitted.

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[*] The then Journal of the Royal Microscopical Society, vol. vii., 1867, p. 253.

[†] Ibid., vol. v., 1865, p. 249.

[‡] 'Einleitung in die Höhere Optik,' p. 48.

[§] American edition, 1857, Boston, vol. xiv., p. 798.

In the 'London, Edinburgh, and Dublin Philosophical Magazine' for September, 1840,[†] the same gentleman published a paper "On the Process of Daguerreotype and its Application to taking Portraits from the Life," in which he describes his attempts to reconcile the chemical and visual foci of portrait objectives, to escape "the effulgence" of the solar rays thrown directly on the sitter, as practised at that time, "abstract from them their heat and take away from them their offensive brilliancy." These are almost the very objects for which microscopists to-day resort to the copper solution. Professor Draper employed in his experiments "a large trough of plate glass, the interstice being an inch thick," filled with a dilute solution of the ammonio-sulphate. Its size was about three feet square. This was so fixed in the course of the sun's rays, reflected from a mirror upon the sitter, that his head and the adjacent parts were illuminated only by the light which had passed through the copper solution. By this device he reports he obtained excellent results.

In the spring of 1869 I received a letter from one of the sons of Professor Draper (dated April 19th), calling my attention to the above facts, and transmitting several daguerreotypes of microscopic objects, all bearing the marks of considerable age. These the writer (Prof. Henry Draper) states were made at various dates from 1851 to 1856. A Nachet microscope was used, and in every case the ammonio-sulphate of copper is said to have been employed.

The results are not particularly good as compared with modern photo-micrographs, but appear to me not much inferior to the best that could have been done by the daguerreotype method with the microscope used. The time was not yet ripe, and both microscopic objectives and photographic methods have greatly improved since those days.

My present purpose does not permit me to give greater space to these reminiscences, the real object of this paper being to indicate the best practical method to be pursued in obtaining economically the advantages of monochromatic sunlight for high-power definition.

This object excludes a further consideration of the use of the prism. It does its work admirably, as I know by repeated trial, but the results are practically no better, even for photography, than those obtained by the use of the ammonio-sulphate cell; it requires greater skill to use, and the necessary apparatus is more expensive. For the same reason I shall say nothing in this article on the use of artificial lights, further than that both the prism and the ammonio-sulphate cell may be satisfactorily used with either the calcium, the magnesium, or the electric lights by those who are unable conveniently to secure the advantages of sunlight. The light of ordinary coal oil or gas lamps, however, is not suitable for the purpose.

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[*] *Loc. cit.*, vol. xix., 1837, p. 473.

[†] Vol. xvii., p. 217.

Two very simple methods of securing the advantages of the ammonio-sulphate solution will now be briefly described.

(*a.*) I suppose the observer to be possessed of a good microscope stand, with achromatic condenser and suitable objectives. Then it is only necessary to prepare a proper ammonio-sulphate cell and fix it between the plane mirror of the instrument and the achromatic condenser. The microscope should be set near a window so that the direct rays of the sun fall on the plane mirror, while the head of the observer is protected by a convenient screen, and all becomes easy.

(*b.*) A still better method for the resolution of lined test-objects with the highest powers, and one which is almost as simple as the foregoing, is that described in my paper "On the Use of *Amphipleura pellucida* as a Test-object for High Powers."*

"Erect a perpendicular wooden screen about two feet square on one edge of a small table. Cut in this a circular hole an inch and a half in diameter at about the height of the under surface of the stage of the microscope. On the outside of this hole mount a small plane mirror which can be adjusted by passing the hand to the outside of the screen. On the inside cover the hole with the ammonio-sulphate cell." Now move the table to a window through which the direct rays of the sun can fall upon the mirror, and adjust this so as to throw the solar pencil nearly horizontally through the ammonio-sulphate cell. The mirror and achromatic condenser, if the microscope has one, are removed and the microscope turned so that the solar pencil shall fall with the desired degree of obliquity on the under surface of the object. It will generally be best to condense the light upon the object by a small ordinary lens, or still better by a low-power objective mounted like a bull's-eye lens on a separate stand.

For prolonged observation, however, the motion of the sun will render it necessary to readjust the mirror from time to time, and the use of a heliostat becomes desirable. This gives the most satisfactory results no doubt, but the cost of the heliostat will, of course, prevent it from coming into general use, except among those who desire to photograph what they observe, and the simpler methods above detailed will answer very satisfactorily for every other purpose.

The ammonio-sulphate cell used in either method should be made of two pieces of thin plate glass about 2½ inches square, held apart by thin strips of plate glass, or by a square of plate glass suitably drilled. The point is to obtain a layer of the blue solution about ⅛ of an inch thick between two parallel planes of plate glass. The best cement for the purpose according to my experience is old Canada balsam applied hot; but many other devices may be employed. The solution is made by saturating strong *aqua ammonia* with sulphate of copper, and should be strained or filtered so as to be free from all solid particles. A sheet of fine blue glass may be substituted for the ammonio-sulphate cell, but only with tolerable results; at least, I have never had a sample of blue glass which was of just the right colour.

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* 'American Naturalist,' April, 1872, p. 193.

The selection of the best condenser for high-power definition is a matter which has recently elicited much discussion. In a general way I may say that any condenser will do its best work under the conditions above indicated if skilfully used. For the benefit of those who possess first-class stands but have never purchased an achromatic condenser, it may be stated that almost any objective suitably mounted on the secondary stage can be made to answer instead, the best results being attained when the angle of aperture of the objective thus used is rather less than that of the one employed to magnify the preparation, and when the secondary stage is capable of being centred or decentred at pleasure by screws working at right angles to each other. An ordinary low-power objective (of 1 to 3 inches focal length), mounted on a separate stand and used to throw the light obliquely as already described, is, however, perhaps the most convenient and efficient mode of illuminating line test-objects with high powers.

As to the objectives suitable for monochromatic illumination, the best compound objectives of some first-class maker should be selected. It is a mistake to suppose, as some have done, that a single lens can be substituted for the modern carefully-corrected compound objective, even if the pure monochromatic light of a narrow portion of the solar spectrum as obtained by a prism were employed. For the objective always requires to be corrected for spherical aberration, and in the case of high powers must be provided with a screw-collar to modify the distance between the posterior combination and the front one in accordance with the different thickness of the covering glass of the preparation. Now practically the spherical aberration is best corrected by the just combination of crown and flint glass, and combinations very nearly the same as those employed for white light would still be necessary if the objective were made for exclusive use with monochromatic illumination.

Under these circumstances I do not recommend the use of monochromatic illumination for low or medium powers except when photographs are to be made. It is only as an aid to high-power definition that I here commend it. With its aid, objectives incapable of resolving certain difficult tests (such as *Amphipleura pellucida*, *Grammatophora subtilissima*, &c.) with white light, show them in a satisfactory manner, and those which even with white light are capable of displaying the most difficult tests, exhibit them with greater clearness and distinctness. I attribute this result chiefly to the well-known fact that the chromatic correction of our very best modern objectives is far from perfect, more or less of a secondary spectrum being always visible, and interfering with distinct vision. Moreover, many of the objects we desire to examine are themselves capable of producing enough chromatic dispersion to interfere with our perception of their true form. Both these evils are escaped by the method here described. I do not advise it as a substitute for other modes of using the microscope, but as a special means of research to be reserved for occasional use in connection with the higher powers of the instrument.

I have frequently been asked to express an opinion as to whether the use of monochromatic sunlight is likely to prove injurious to the eye of the observer. On this subject I can speak from an extensive personal experience in connection with photo-micrography. The only injury to my own eyesight of which I have ever been conscious was produced by an injudicious exposure to the electric lamp. If the microscopist so manages his illuminating apparatus that the field of the microscope resembles in colour and intensity the azure blue of the sky on a clear day (and this is the condition which should always be aimed at), I do not believe the use of the method for any reasonable time will be found injurious. I have recently found, when a sheet of plate glass backed with black velvet is substituted for the ordinary plane mirror in any of the above arrangements, that while the brilliancy of the light is much moderated, its desirable qualities are unchanged, and it is still intense enough for the adequate illuminating of the highest powers. Those who find the light obtained from the ordinary mirror too brilliant may resort to this contrivance with advantage.—*Paper read before the Philosophical Society of Washington, U.S.A.*

VII.—*One of our Common Monads.*

By Professor Albert H. Tuttle.

Since the investigations of Clark, Carter, and others upon the sponges and their allies, anything which adds to our knowledge of the Flagellate Infusoria is of especial interest. I have fortunately had an opportunity of making a careful study of a genus *Urella* of

Ehrenberg, about which thus far but little that is satisfactory has been known.

Until recently but little has been known of the real nature of the Monadina. As a natural consequence, organisms have been referred to the different genera of this family upon very slight study and superficial resemblances. Urella, which Ehrenberg describes thus, "aggregate monads, free-swimming, tailless, devoid of an eyespeck, and having one or (doubtfully) two flagelliform filaments," has of course received its share of attention, until one might think from the "species" and the figures given that the diagnosis of the genus had been "anything very minute, aggregated, free-swimming,"—whether Infusorian or Alga making no difference.

When, therefore, I found in a collection made at Spy Pond, near Cambridge (U.S.A.), on the 25th of November last, a large number of individuals of this genus (probably the species *glauconia* Ehr.), I made use of the opportunity for a careful study of it, devoting my spare time to it daily as long as I continued to find it in the water; what follows is therefore the result of a number of observations, at which each point has been examined and verified.

Urella probably finds its nearest ally in Anthophysa, differing from that genus principally in being free-swimming instead of fixed upon a stalk. The number of monads in a colony is quite variable, almost every number having been seen, up to forty or fifty; in this, however, as in many other respects, the constant activity of the colonies renders it impossible to speak with absolute certainty; even when cornered, so that they could no longer progress in the direction in which they had been moving, they continued to revolve upon their axes with considerable rapidity, making it impossible to count them with accuracy. Occasionally a group of five or six, or even two or three, and not unfrequently a single monad, would be seen, and these were more available for purposes of study, though the larger groups were more frequent.

From such measurements as I was able to make while they were in motion, I should say that the average length of each monad was about $\frac{1}{2000}$th and the breadth $\frac{1}{5000}$th of an inch; but these dimensions varied a good deal with the size of the colonies, the individuals in the larger groups being more elongated and narrower than those in the smaller ones. The form may be described as conical, with a rounded base not at right angles with the axis of the cone, the part at the greatest distance from the apex being the one nearest the apex of the colony; in colonies of over ten or twelve the axis of the cone being also bent towards the apex of the group, especially in those Monads near the base. The form of the larger colonies varied from hemispherical to raspberry or even mulberry shape.

I was unable to find any trace of a common investing membrane in either the larger or the smaller groups, nor do I believe that any such membrane exists in any true species of Urella.

I was able to distinguish clearly *two* flagella, both arising from a point near the most elevated side of the base of the cone. The larger one was stiff, arcuate with the concave side toward the base before mentioned, somewhat longer than the body of the Monad, and very plainly seen with any objective above a $\frac{1}{4}$-inch; the smaller is very delicate, short, and incessantly active. I first saw it with a Wales' $\frac{1}{15}$th; but after becoming familiar with its appearance, I was always able to detect it with a Tolles' $\frac{1}{8}$th or a Hartnack No. 9.

I am not certain as to the number of contractile vesicles, but it is my opinion that there is but one: sometimes a larger number of clear spaces would be seen in the body, but they were not observed to contract. I do not wish, however, to speak with any degree of positiveness upon this point, as I was not able to keep a single Monad in view long enough to satisfy myself, on account of the constant revolution of the colonies. As regards the ingestion of food, I have seen something, though not as much as I could wish, as this is still a disputed question. I fed them with indigo, which they ate readily, and I frequently watched its ingestion. On account of their incessant motion, I was not able to satisfy myself of the existence of a definite mouth, but I did not see a single instance of the indigo being received at any point except very near the common base of the flagellum, and in every instance observed the act of ingestion was preceded by a quick bending of the larger flagellum, by which the particle of indigo was thrown against the surface of the body in a manner similar to that described by Professor Clark in his observations upon Monas. So far as I was able to follow the process, whenever a colony reduced its rate of motion sufficiently to permit of careful observation upon this point, it was so much like the process described in Monas as to leave little doubt in my mind that it was substantially the same, although, as I have said, I did not *see* a distinct mouth.

It appears probable, now that the Monadina are better understood, that we shall soon be able to recognize in them a well-defined family of the Flagellate Infusoria, although, doubtless, many forms that have been assigned to that group are vegetable in their nature. These will be gradually removed, and those forms which are unquestionably animal will be distinguished. Among these it seems to me the genus Urella, as described by Ehrenberg, will undoubtedly take its place.—*A paper communicated to the Section Microscopy of the Boston Society of Natural History.*

NEW BOOKS, WITH SHORT NOTICES.

The Micrographic Dictionary: a Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. Third edition. By J. W. Griffiths, M.D., assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and Rupert Jones, F.G.S. London: Van Voorst. 1872. Parts III., IV., V., VI., VII., and VIII.—This excellent work continues, though slowly, to make its appearance, and we must say that though in some few instances the text is not sufficiently altered or added to, in most cases the additions made in point of reference sufficiently counterbalance this otherwise defect. This is shown more particularly under the head of Angular Aperture, in which we find an omission of most of the facts relating to the aperture highest and lowest of immersion glasses. This, however, will be forgiven when the reader remembers that for a great many microscopists it is still an undecided point, and also from the fact that all the references to the several papers which have appeared in this journal are to be found given. Notwithstanding this want of reference to immersion lenses, the article on Angular Aperture is fully given and the explanations are very plainly stated. The classification of the animal kingdom, however, requires revision; the term Radiata should not have been admitted in so late a publication, and the Cœlenterata should not have been given such an inferior position; Annulosa too should have taken the position of Articulata, and the Bibliography might have been more fully given. The section of Aphides seems to have been carefully done, and so also are Arachnida, Arteries, Ascomycetes, Batrachospermum; Blood corpuscles, as to their measurement and constitution; Bone, capitally illustrated; Bursaria; Cambium; Cell, very good, if not best in the volume, but that reference to Huxley's able paper in the 'Med. Chir. Rev.' is omitted; Chalk, excellent; Characeæ, Chlorophyll, Ciliæ, Closterium, Coccoliths, Coal, and lastly Confervoideæ, which completes the eighth number, but is not yet finished. These articles are all admirable, and where information is not distinctly given, the references to it are abundantly supplied, so that the reader may inform himself to the utmost on each subject which he takes up. Of course, there are deficiencies in some of the papers, but then we cannot consider these till the volume is completed, as there are many occasions of giving information which might or might not be placed under a single heading. All we can say is that the work so far meets our desires, and that we doubt not, it will be even better in the future numbers.

PROGRESS OF MICROSCOPICAL SCIENCE.

The Mode of Existence of the Dendroid Graptolites.—In a paper on the Graptolites of the Arenig Rocks of St. David's, read before the Geological Section of the British Association at Brighton, Mr. John Hopkinson mentioned the occurrence of a new species of *Callograptus*, with an entire "hydrocaulus," or main stem, and a "hydrorhiza"—the organ of attachment. This appears as a series of interlacing fibres at the proximal end of the main stem, which suddenly widens out and becomes more loose or fibrous in structure as it merges into the adhesive fibres. The genus *Callograptus* is thus proved to have been capable of attachment to foreign bodies, and it may therefore be inferred that its near allies, *Ptilograptus*, *Dendrograptus*, and *Dictyonema*, were also provided with hydrorhizæ. *Ptilograptus* and *Dendrograptus* being thus, in general form, in mode of growth, and most probably also in structure, essentially similar to our recent Hydroida; *Callograptus* and *Dictyonema* differing only in their branches, being more or less regularly connected together by transverse processes. For this species the name *C. radiculatus* is proposed, to distinguish it as the first in which these radicular fibres have been seen.

The Nature of Diatom Structure.—Some researches on this point have been lately carried on by Professor Weiss, of Lemberg, who regards the silicious envelope as capable of polarizing light, and as consisting of a cellulose coat more or less infiltrated with silex. He does not consider the individuals one-celled, but finds the valves composed of cells from ·008 to ·00025 mm. in diameter. These cells are furnished at their centres with papillæ, which appear as striæ under low powers, and as moniliform markings under high powers. The large cavity between the frustules is regarded as equivalent to the embryo-sac of higher plants, and the formation of new individuals has been observed within it. An alternation of generation is indicated by the observations made.

The True Markings of the Test Podura Scale.—An interesting paper on this subject has appeared in the July number of the American 'Lens.' From the latter journal we take the following account, which, though somewhat long, is incapable of being abstracted. In the first place, says the author, Dr. Arnold, of New York, I will briefly state my views, as expressed in June, 1871. There are, I believe, some species of Podura that are scaleless, and are clothed with hairs—compound hairs; that is, the hairs are covered by projections, usually in considerable number at *one* end, where they form a brush-like arrangement. In fact, all the species of Podura that I have met with possess analogous hairs to a greater or less degree. It is, then, my purpose to try and establish some points of similarity in structure between the hairs and scales themselves. If we agree that these compound hairs are covered by epithelial scales, and are hairs of other animals, why should not the "*scales*" also be covered with like structures? As it is my belief at the present time that in the Test Podura scale, as well as that of the Degeeria, the structure of hair and scale is alike, I will give the points which have caused this opinion. There is no doubt that the epithelial scales on the hairs of *L. curvicollis* and *Degeeria domestica* bear a great similarity to the markings on the scales themselves. They are of the same shape, and I have in several instances made measurements which showed the coincidence in size between the "spines" on the hairs and those of the scales. It is comparatively easy to obtain isolated spines from the hairs, and these, when viewed by central illumination, present, as I have stated, an appearance identical with the so-called spines of the scales. If unilateral illumination be employed, a beading is also seen on the spines separated from the hairs, although I have never been able to produce the "varicosities" as well as when they were attached to and lying upon the hair itself. Having satisfied myself as to this fact, the next thing to do was to separate the spines from the scales. I accidentally came into possession of a slide of *Degeeria domestica* scales, mounted by J. Beck, of London, I believe. Here, on this slide, I perceived to my great satisfaction a fractured scale, which showed the spines projecting beyond the broken edge, some of them bent and distorted, and what were evidently places from which others were torn. At a short distance from the broken scale I beheld *isolated spines* just like those on the scale, the measurements of which corresponded to the size of those attached to the scale. These separate spines I have repeatedly seen as rows of beads when unilateral illumination was resorted to, and the projecting spines from the broken margin of the scale exhibited the beading under similar conditions. Dr. J. J. Woodward, of the Army Medical Museum, Washington, on receiving from me some photo-micrographs that I made of the appearances with central light, wrote me that he was of opinion that the free spines came from some of the compound hairs that might be upon the slide. Now, even granting this to be so, the *projecting spines*, their being beaded under oblique light, the beading of the free spines, seem to point to the fact that there is something yet to be accounted for that the theory of beads leaves untouched. This, then, represents the state of the case and conclusions which I incorporated in my paper nearly a year ago. Since then I have secured some specimens of Degeeria, and continued my experiments,—imperfectly, it is true, as time could not be spared for exhaustive research in this direction. I selected specimens of scales that were not mixed with hairs, and that showed no free spines, and, placing these in such a manner that the discharge from a Leyden jar could be brought to bear upon them, not only tore to pieces the scales, but scattered the spines far and wide. Again, by means of crushing in a small agate mortar, and even on the slide itself, by pushing about the covering glass with the handle of a dissecting needle, I have fractured a scale in such a manner that the spines lay free and side by side with the broken scale. What more is wanting to show that hairs and scales are of a similar structure?—the hair consisting of a shaft, upon the surface or exterior of which are attached spine-like epithelial cells; the scale of a flattened shaft, so to speak, covered by analogous structures. I may state that the scales or even hairs from the live insect are not in a fit condition to break; they are too pliable and tough, so that I have not succeeded in doing anything in the way of breaking when the scales were in this state. By carefully drying in an ordinary drying oven, such as is used in chemical laboratories for drying precipitates, &c., the scales become much more brittle, and the greasy substance that can be pressed from them while fresh entirely disappears through the agency of a gentle and continued heating. It will require a discharge of some force to break off the spines, and several discharges in rapid succession will give best results. As it is desirable to examine the scales immediately after the action of the spark, some arrangement similar to that used in Germany for acting upon tissues with the induced current is most convenient. A rather thick glass slide, with two pieces of tin-foil pasted upon it at a distance apart just sufficient to allow of the passage of the spark, with the scales in the pathway of the discharge covered by another slide or strong cover glass, will make a simple and effective instrument. It must not be supposed that the blowing off of the spines is to be accomplished at every attempt. I find that when the spark passes across the scale from side to side, so as to strike the spines on their lateral surfaces, the chances are best of rending off the spines. Many attempts must fail, and many a fine "Test Podura" will have to be sacrificed, as the scales, either untouched or in fragments, will be oftentimes scattered to such an extent that only here and there on the slide can one be found; and then to mount the individual on a clean slide with thin cover, in order to examine with higher powers, is a very difficult matter.

The Anatomy of Balantidium coli.—The 'Northern Archives of Medicine' contains a paper by *M.* P. J. Wising, of Stockholm, on the anatomy and physiology of *Balantidium coli*, a parasite first described by Professor Malmsten in 1856. In 1869 a labourer, Lars Nilsson, sixty-two years of age, noticed for the first time the presence of small white worms in the motions from his bowels. About Christmas of that year he suddenly became affected with severe colicky pains, followed by considerable diarrhœa. This at last became so bad that there were as many as twelve stools a day; they were usually of the consistence of thin pea-soup, sometimes with firmer lumps intermixed, sometimes quite watery, and most frequently tolerably small in quantity. There was but slight tenesmus, while after some time the stools were streaked with blood. The microscope showed, in addition to the remains of food, a good many lymphoid cells, a few red blood-corpuscles, vibriones, and *Balantidium coli* in very large numbers. The more original researches of the author are directed to the question of the reproduction and propagation of the parasite. In a series of beautifully-executed lithographs the anatomical structure of the animal, including the peristome, with its adoral ciliæ, nucleus, nucleolus, and contractile vacuoles, the phenomena of coition and the process of reproduction, are represented. The last-named is carried on in this way. The body of the parasite elongates so that its length becomes more than double its breadth, and the nucleus itself, also somewhat elongated, assumes a central position, with its long axis in the direction of that of the body. A tendency to constriction across the middle of both body and nucleus is the next change observed. This increases until the two halves are at last connected by a tiny filament only. The anterior half contains the old peristome quite unaltered, while in the posterior half new adoral ciliæ begin to develop from the ordinary ciliary layer. At this stage the nucleus parts in two, so as to form a separate nucleus for each half of the body. Usually only one contractile vacuole is now found lying near the back portion of each half, for the anterior vacuole does not generally appear until the separation has been quite completed; both halves of the animal gradually take on a more independent life, and, finally, the connecting filament gives way, and the two halves fall apart as distinct living beings. The anterior half, which contains the original peristome, is a fully-developed animal from the time of the separation; but the posterior half is as yet very unlike the full-grown parasite, into which it in time develops.

Bacteria in the Uterus.—Professor Waldeyer, of Breslau, records that he has found these in great numbers in the diphtheritic deposit on the inner surface of the uterus, the puriform mass from the lymph-vessels of the uterus and broad ligaments, the peritoneal exudation, both in the fluid and also in the whitish-yellow purulent fibrinous flakes. Haussmann formerly only found the Bacteriæ and vibriones in the lochial secretion. In the diphtheritic exudation these Bacteriæ lie between the pus-cells and those of the partly decomposed tissue elements. The puriform contents of the lymph-vessels consist of, besides the pus-cells, for the most part of Bacteriæ, and they are also found in the peritoneal exudations and flakes, mixed with pus-cells, young cells, and masses of fibrine, and even in the interior of the pus-cells.

Opaque Illumination under High Objectives.—Dr. W. A. Johnson has communicated to the Microscopical Society of Illinois another mode of managing the above. He employs Professor H. L. Smith's mode of making the objective its own illuminator; but instead of the silvered reflector, thin glass disk, or reflecting prism, used by Professor Smith and subsequent experimenters, he employs the binocular prism of Wenham's binocular. A beam of light reflected down the oblique body of the Wenham binocular, the prism being in position as for ordinary use, is of course condensed by half the objective upon the object in its focus; while the other half of the objective and direct body of the instrument is used at the same time as a monocular for viewing the object. A portion of the field is intensely illuminated, and definition is said to be better than with other arrangements.—*American Naturalist.*

The Internal Limiting Membrane of the Retina.—A paper on this subject appears in the 'Northern Archives of Medicine.' The author, *M.* G. Retzius, describes peculiar supporting filaments with trumpet-shaped terminal disks. He has in the same journal a second paper, "On the Inner Layer of the Connective Tissue of the Eye." Both papers are of interest.

The Microscopic Anatomy of Skin Grafting.—In Part iv. of the 'Proceedings of the Swedish Society of Physics,' M. Misterton gives the results of six cases of skin grafting. He says that in all these, even where the transplanted piece of skin seems to disappear completely, the operation succeeds if only a few cells of the Malpighian layer of the cuticle remain possessed of life, so as to form a nucleus for a fresh epidermis formation. The operation of skin grafting is graphically and physiologically described in the paper.

The Embryology of Fossil Cephalopoda.—In vol. ii., No. 5, of the 'Bulletin of the Museum of Comparative Zoology' (U.S.A.), Mr. Alpheus Hyatt describes the results of a careful investigation with respect to the embryology and structure of ammonites and related cephalopods, by a study of the shell in its different stages of development, and also by a comparison of its characters with those of the living nautilus.

The Eozoon: Dr. Dawson and Professors King and Rowney.—Our readers are perhaps aware that Messrs. King and Rowney have attempted to prove that this unquestionably animal form was merely a peculiar mineral. They wrote a long paper setting forth their views. This has now been replied to by Dr. Dawson, and his answer is to be found in substance in 'Silliman's American Journal' (July, 1872). He (Dr. Dawson) says, that in opposition to the facts stated by him, the authors of the paper under consideration maintain that the structures are mineral and crystalline. "I believe that in the present state of science such an attempt to return to the doctrine of 'plastic force' as a mode of accounting for fossils would not be tolerated for a moment, were it not for the great antiquity and highly crystalline condition of the rocks in which the structures are found, which naturally create a prejudice against the idea of their being fossiliferous. That the authors themselves feel this is apparent from the slight manner in which they state the leading facts above given, and from their evident anxiety to restrict the question to the mode of occurrence of serpentine in limestone, and to ignore the specimens of Eozoon preserved under different mineral conditions. With reference to the general form of Eozoon and its structure on the large scale, I would call attention to two admissions of the authors of the paper, which appear to me to be fatal to their case. First, they admit, at page 533,* their 'inability to explain satisfactorily' the alternating layers of carbonate of lime and other minerals in the typical specimens of Canadian Eozoon. They make a feeble attempt to establish an analogy between this and certain concentric concretionary layers; but the cases are clearly not parallel, and the laminæ of the Canadian Eozoon present connecting plates and columns not explicable on any concretionary hypothesis. If, however, they are unable to explain the lamellar structure alone, as it appeared to Logan in 1859, is it not rash to attempt to explain it away now, when certain minute internal structures, corresponding to what might have been expected on the hypothesis of its organic origin, are added to it? If I affirm that a certain mass is the trunk of a fossil tree, and another asserts that it is a concretion, but professes to be unable to account for its form and its rings of growth, surely his case becomes very weak after I have made a slice of it, and have shown that it retains the structure of wood. Next, they appear to admit that if specimens occur wholly composed of carbonate of lime their theory will fall to the ground. Now such specimens do exist. They treat the Tudor specimen with scepticism as probably 'strings of segregated calcite.' Since the account of that specimen was published, additional fragments have been collected, so that new slices have been prepared. I have examined these with care, and am prepared to affirm that the chambers in these specimens are filled with a dark-coloured limestone not more crystalline than is usual in the Silurian rocks, and that the chamber walls are composed of carbonate of lime, with the canals filled with same material, except where the limestone filling the chambers has penetrated into parts of the larger ones. I should add that the stratigraphical researches of Mr. Vennor, of the Canadian Survey, have rendered it probable that the beds containing these fossils, though unconformably underlying the Lower Silurian, overlie the Lower Laurentian of the locality, and are, therefore, probably Upper Laurentian, or perhaps Huronian, so that the Tudor specimens may approach in age to Gümbel's *Eozoon Bavaicum.* Further, the authors of the paper have no right to object to our regarding the laminated specimens as 'typical' Eozoon. If the question were as to *typical ophite*, the case would be different; but the question actually is as to certain well-defined forms which we regard as fossils, and allege to have organic structure on the small scale, as well as lamination on the large scale. We profess to account for the acervuline forms by the irregular growth at the surface of the organisms, and by the breaking of them into fragments confusedly intermingled in great thickness of limestone, just as fragments of corals occur in Palæozoic limestone; but we are under no obligation to accept irregular or disintegrated specimens as typical, and when objectors reason from these fragments we have a right to point to the more perfect examples. It would be easy to explain the loose cells of *Tetradium*, which characterize the Bird's-eye limestone of the Lower Silurian of America, as crystalline structures; but a comparison with the unbroken masses of the same coral shows their true nature. I have for some time made the minute structure of Palæozoic limestones a special study, and have described some of them in the Trenton formation of Canada. I propose shortly to publish additional examples showing fragments of various kinds of fossils preserved in these limestones, and recognizable only by the infiltration of their pores and other minute structures. I shall also be able to show that in many cases the crystallization of the carbonate of lime and the infiltration of other substances have not interfered with the perfection of the most minute of these structures. The fact that the chambers are usually filled with silicates is strangely regarded by the authors as an argument against the organic nature of Eozoon. One would think that the extreme frequency of silicious fillings of the cavities of fossils, and even of silicious replacement of their tissues, should have prevented the use of such an argument, without taking into account the opposite conclusions to be drawn from the various kinds of silicates found in the specimens, and from the modern filling of Foraminifera by hydrous silicates, as shown by Ehrenberg, Mantell, Carpenter, Bailey, and Pourtales.* Further, I have elsewhere shown that the loganite is proved by its texture to have been a fragmental substance, or at least filled with loose *débris;* that the Tudor specimens have the cavities filled with sedimentary limestone, and that several fragmental specimens from Madoc are actually wholly calcareous. It is to be observed, however, that the wholly calcareous specimens present great difficulties to an observer; and I have no doubt that they are usually overlooked by collectors in consequence of their not being developed by weathering, or showing any obvious structure in fresh fractures. With regard to the canal system, the authors persist in confusing the casts of it which occur in serpentine with 'metaxite' concretions, and in likening them to dendritic crystallizations of silver, &c., and coralloidal forms of carbonate of lime. In answer to this, I think it quite sufficient to say that I fail to perceive the resemblance as other than very imperfect, imitative. I may add that the case is one of the occurrence of a canal structure in forms which on other grounds appear to be organic, while the concretionary forms referred to are produced under diverse conditions, none of them similar to those of which evidence appears in the specimens of Eozoon. With the singular theory of pseudomorphism, by means of which the authors now supplement their previous objections, I leave Dr. Hunt to deal. With regard to the proper wall and its minute tubulation, the essential error of the authors consists in confounding it with fibrous and acicular crystals, and in maintaining that because the tubuli are sometimes apparently confused and confluent they must be inorganic. With regard to the first of these positions, I may repeat what I have stated in former papers—that the true cell-wall presents minute cylindrical processes traversing carbonate of lime, and usually nearly parallel to each other, and often slightly bulbose at the extremity. Fibrous serpentine, on the other hand, appears as angular crystals, closely packed together, while the numerous spicular crystals of silicious minerals which often appear in metamorphic limestones, and may be developed by decalcification, appear as sharp angular needles usually radiating from centres or irregularly disposed. Plate 44, Fig. 10,† is an eminent example of this; and whatever the nature of the crystals may be, they have no appearance in the plate of being tubuli of Eozoon. I have very often shown microscopists and geologists the cell-wall along with veins of chrysotile and coatings of acicular crystals occurring in the same or similar limestones, and they have never failed at once to recognize the difference, especially under high powers. I do not deny that the tubulation is often imperfectly preserved, and that in such cases the casts of the tubuli may appear to be glued together by concretions of mineral matter, or to be broken or imperfect. But this occurs in all fossils, and is familiar to any microscopist examining them. How difficult is it in many cases to detect the minute structure of Nummulites and other fossil Foraminifera. How often does a specimen of fossil wood present in one part distorted and confused fibres or mere crystals, with the remains of the wood forming phragmata between them, when in other parts it may show the most minute structures in perfect preservation. But who would use the disintegrated portions to invalidate the evidence of the parts better preserved? Yet this is precisely the argument of Professors King and Rowney, and which they have not hesitated in using in the case of a fossil so old as Eozoon, and so often compressed, crushed, and partly destroyed by mineralization. I have in the above remarks confined myself to what I regard as absolutely essential by way of explanation and defence of the organic nature of Eozoon. It would be unprofitable to enter into the multitude of subordinate points raised by the authors, and their theory of mineral pseudomorphism as discussed by my friend Dr. Hunt; but I must say here that this theory ought, in my opinion, to afford to any chemist a strong presumption against the validity of their objections, especially since it confessedly does not account for all the facts, while requiring a most complicated series of unproved and improbable suppositions. The last point which I shall mention is the taunt that so little further progress has been made in the investigation of Eozoon. With reference to this, I beg leave to doubt whether a process of confounding the actual structure of Eozoon with all manner of dendritic and crystalline forms, in the way followed by the authors, would constitute progress. But in so far as careful comparison with all specimens which have been recently found is concerned, some progress has been made, and I trust that it will soon be possible to bring forward not merely additional specimens illustrative of the structure of Eozoon, but fresh evidence of its wide geographical range, and also links of connection with fossils of the Palæozoic rocks. The discovery recently made in Massachusetts, and alluded to by Messrs. Rowney and King, is itself not without importance. In the meantime I am content to accept the investigations of Messrs. King and Rowney as nearly exhaustive of the natural history of those imitative forms which may be confounded with Eozoon; and therefore as in a certain way useful in the further prosecution of the subject. As already stated, I am at this moment engaged in following out, as opportunity offers, two lines of investigation bearing on the following points:—(1) The study of the Lower Silurian and Primordial successors of Eozoon; and (2) that of the tubulation and other structures similar to those of Eozoon preserved in the Palæozoic rocks."

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* 'Proceedings R. I. A.,' vol. x.

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* 'Quart. Journ. Geol. Soc.,' 1864.

† Ophite from Skye, King and Rowney's paper, 'Proc. R. I. A.,' vol. x.

The Histology of the Penis.—One of the best papers that we have for some time seen is that on the above subject, in a recent number of the 'New York Medical Journal.' It deals very fully with the whole subject, and is illustrated by a number of cuts demonstrating the more important points in the histology of this organ. Besides the author's own opinions on the subject, he has given those of the most recent foreign authorities, and thus has given the reader a useful summary of the work done on this subject. We hope, when the blocks have been received, to lay the paper fully before our readers.

The precise period at which the Laminar Membrane appears.—Dr. Joulin, who has been known for his researches on the placenta some years since, presented to the Academy of Medicine a memoir in which he endeavoured to show that the chorion at term did not, as is generally described, cover the fœtal surface of the placenta, but that it was replaced by a membrane of new formation called the "laminar membrane." The object of his present communication is to show the precise period at which the chorion disappears, when new membrane takes its place. The conclusion he arrives at is that the chorion begins to disappear from the fœtal surface of the placenta about the tenth or eleventh week of utero-gestation, the new membrane being formed at first in connection with the great vessels, and gradually extending over the entire placenta. Microscopically it consists of fibres in distinct layers, and can be readily distinguished from the other membranes surrounding the fœtus.

The Structure of the Uterus has been very fairly investigated by Dr. Snow Beck, and his researches will be found more fully stated in the 'Obstetrical Transactions' (vol. xiii.). In his paper he advances views somewhat different from those generally taught. He does not admit the existence of a separate mucous membrane lining the uterus. He describes the walls of the uterus as consisting of two parts:—(*a*) The contractile tissue destined to expel anything contained in the uterine cavities, and which exists in greatest amount at the outer and middle portions, whilst towards the inner surface it is divided into smaller striæ with considerable spaces between them. (*b*) The soft tissue which lines the internal surface, where it exists alone to some thickness, and afterwards penetrates a considerable distance into the substance, lying between the striæ and bands of the contractile tissue. It is destined to form the placenta which is thrown off at the completion of parturition, together with the inner layer from the whole of the inner surface of the uterus, as decidua. But it is only the inner layer of the soft tissue which comes away, the principal portion yet remaining in the inner portion of the walls. The decidua and the formation of the placenta appear to be the natural development of the soft tissue which exists at the inner surface of the unimpregnated uterus, and at the completion of parturition it is thrown off from the surface as now useless products, and there does not appear any sufficient reason to apply special designations to particular portions of it.

NOTES AND MEMORANDA.

Angular Aperture of Immersed Object-glasses.—Mr. Wenham having expressed his willingness to assist in testing the aperture of Mr. Tolles' object-glass, which he announced in his letter to the Editor, dated 15th July,* would be sent, in order to have the result verified or not, we have to announce that the glass has not arrived. Though Mr. Wenham has declined further discussion directly with Mr. Tolles, we think that the subject has not been quite satisfactorily determined on the question of the discrepancies, or perhaps fallacies of measurement of extreme angular apertures. If Mr. Wenham holds this opinion, we hope that he will give us the result of his experience, irrespective of any individual question of controversy.

The British Association.—At the late meeting the amount of microscopical work brought before the Association was neither large nor important. And this we should not have been led to expect from the fact that the President for the year was a man who especially stands high among English, and we might add Continental naturalists, as a microscopist and a master of histology. Indeed, with the exception of the President's Address, and a few of the special addresses, there was not much to be noted. Dr. Carpenter's opening remarks were, as might have been anticipated, a masterly summary of the present condition of science. And while he left nothing untouched, he did not fail to comment on the readiness with which men in special departments were ready, not only to believe in particular conclusions drawn from very recent experiments, but were ever disposed to go beyond them, and to give absolute credence to a multitude of other mere speculations which took their rise in them. We think the President touched an important subject in these remarks, and we doubt not that the tendency will be much lessened for the future. We believe the Address may still be had by communication with the Secretaries.

A Prize for Microscopic Work is offered by the Royal Danish Society, and we trust some of our Fellows will attempt competition for it. It is now a hundred years since the celebrated observations of Müller on agamous reproduction (gemmiparity) of the Naiades was published, and although there is no reason to question their perfect accuracy on all essential points, it is very desirable to have them resumed in the actual light of science and with the means which it possesses to-day. Schultze, Leuckart, and Minor have furnished history with valuable contributions of the manner of reproduction of the Naiades properly called, as Claus and Lankester have of Chætogaster; nevertheless, science ought to be in possession of materials sufficient for intelligence on all points of which it is necessary to keep account. It is not known definitely what is the first origin of buds or first individuals, and the relations between the modes of gemmiparous and scissiparous reproduction consequently need to be better explained; complete evolution, from the moment when one Naiad leaves the egg to that in which, among the generations sprung from that Naiad, there are found again sexual distinctions, has not been studied in all its phases, and it is still a question whether the same individuals (zoöides) are gemmiparous and sexual, or if the sexual and agamous reproductions are strictly distributed over different individuals or generations. As for the other two groups of Anellides in which agamous reproduction has been observed hitherto, namely, the Syllides and the Sepulides, the question is almost at the same point. For these reasons, the Society desires to urge a thorough research, and one answering the actual demands of science, of agamous reproduction and all the points pertaining to it, of one of the groups of these setiferous Anellides. It therefore offers its gold medal as a prize to the one who shall solve this question in a satisfactory manner, either for one or several species of the group of Naiades (comprising Chætogaster) or for one or several species of Syllides or Tubicolides. The papers should be accompanied by the necessary drawings explaining the points on which the researches have especially borne. The answers to these questions may be written in Latin, French, English, German, Swedish, or Danish. The papers must not bear the name of the author but a motto, and must be accompanied by a sealed note, furnished with the same motto, enclosing the name, profession, and address of the author. The members of the Society who live in Denmark do not take part in the competition. The prize awarded for a satisfactory answer to the question proposed is the gold medal of the Society (value, about 20*l*.). Papers must be addressed before the end of October, 1873, to the Secretary of the Society, Counsellor J. Japetus Sm. Steenstrup, Copenhagen.

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* 'M. M. J.,' page 147, July, 1872.

Photographing Amphipleura pellucida.—Dr. J. J. Woodward gives, in a late number of the 'American Naturalist,' an able paper, accompanied by a photographic plate of great excellence, on the subject of photographing *A. pellucida*. After describing other means of conducting the operation, he states that that by sunlight involves least trouble and expense, and may be best managed as follows:—Erect a perpendicular wooden screen about two feet square on one edge of a small table. Cut in this a circular hole an inch and a half in diameter at about the height of the under surface of the stage of the microscope. On the outside of this hole mount a small plane mirror which can be adjusted by passing the hand to the outside of the screen. On the inside, cover the hole with the ammonio-sulphate cell. (A piece of dark blue glass will answer the purpose, though not so well). Now move the table to a window through which the direct rays of the sun can fall upon the mirror; adjust this so as to throw a nearly horizontal pencil of parallel rays through the hole, and place the microscope in the shade of the screen in such a position that the parallel blue rays will fall on the under surface of the Amphipleura slide at an angle of from 50° to 75° with the plane of the slide (I suppose the frustule to be examined has first been found by ordinary daylight or lamplight). Next place a small bull's-eye or any other condenser of from one to three inches focal length (mounted on a separate stand or on a radial arm) in the parallel pencil in such a position as to concentrate the light, at the angle above indicated, upon the frustule under examination. After this nothing remains but to regulate the cover correction and the fine adjustment. The precise angle which should be given to the illuminating pencil will vary with the angle of aperture of the objective used. As a rule it should be less than half the angle of aperture of the objective, and 70° to 75° is the maximum angle which should be given even for objectives of 170° angle, a greater angle distorting the image without improving the definition. The same results can be obtained by using a heliostat to fix the direction of the solar rays, and obtaining obliquity by an achromatic condenser of from 130° to 150° suitably decentred. On account of the stability of the illumination this method is especially suitable for photographing the Amphipleura, but the simpler method above described answers every purpose if the object is to compare objectives.

An Optical Instrument of Mr. Tolles's Device.—The 'Lens' states that Mr. Tolles has just made to order a novel instrument. It consists of a vertical tube, seven-tenths of an inch in diameter inside, twelve inches high, supported on a base about two inches square. On one of the vertical faces of the base is a compound achromatic telescope lens three-quarters of an inch in diameter. Between the lenses is a total reflecting prism; at the upper end of the tube a solid ocular. Looking down the tube, the observer can obtain a well-defined image, magnified thirteen diameters, of an object distant three feet horizontally from the lens. "As the term micro-telescope has been applied to Tolles's instruments where one tube and ocular serves for both microscope and telescope, this instrument may be called a telemicroscope."

The Paraboloid an Immersion Instrument.—Notwithstanding the introduction into use of special contrivances as immersion paraboloids, it may not have occurred to all who use the microscope that the ordinary form of parabolic illuminator is capable of being used wet with excellent results. Placing the microscope in a vertical position, says T. D. B. in the 'American Naturalist' for August, and greasing the rod in the centre of the paraboloid to keep the water from running out by the side of it, the cup of the paraboloid is filled with water heaped up as far as can be without running over, and then brought up until the water comes in contact with the under surface of the slide. The direction of the rays leaving the paraboloid is not altered by this arrangement, but dispersion at two surfaces is avoided and the rays enter the object-slide without the usual refraction, and at such an angle as to suffer total internal reflexion before reaching the objective. With the highest objectives generally used with black-ground illumination, as a $\frac{1}{4}$th of 75° to 110°, the object seems no brighter than usual, but the field is free from the foggy diffuse light, otherwise present, and the object appears, beautifully distinct, upon a jet-black ground. Even a $\frac{1}{6}$th or $\frac{1}{8}$th of 130° gives the same effect of a deep black background, and shows the object with good stereoscopic effect in Wenham's binocular. With objectives of 170°, the main effect is that of a dark background, though not so perfect as with the lower angles.

CORRESPONDENCE.

Correction of an Error.

To the Editor of the 'Monthly Microscopical Journal.'

Troy, New York, *July* 30, 1872.

Dear Sir,—Please make the following correction to page 17 of your July number. In fifth line of note, for "applications" *read* appliances.

Yours truly,
R. H. Ward.

PROCEEDINGS OF SOCIETIES.

Brighton and Sussex Natural History Society.

The Microscopical and Botanical Displays at the Soirées of the British Association Meeting at Brighton.

The executive committee of the British Association having asked the Society to organize the microscopic displays at the soirées of Thursday, August 15th, and Tuesday, August 20th, and to arrange, as far as practicable, a living flora of the county for the soirées and temporary museum, the following gentlemen were appointed a committee to carry out the first, *viz.* Dr. Hallifax; Messrs. Hennah, Sewell, C. P. Smith, Hazelwood, W. H. Smith, F. E. Sawyer, G. Scott; the President, Mr. Hollis; and the Hon. Secretaries, Messrs. Wonfor and Onions; and to arrange the flora, Messrs. W. Mitten, Nourse, Simonds, C. P. Smith, W. H. Smith, Benjamin Lomax, D'Alquen, G. Davies, W. H. Hallett (Local Treasurer of the British Association), H. J. H. Nicholls, and the officers of the Society were elected a committee.

The microscopical committee feeling that the leading feature of one of the soirées ought to be marine, drew up the following suggestive programme:—

British Association for the Advancement of Science.

Brighton Meeting.

Particulars of the intended Microscopical Displays for the Soirées arranged by the Brighton and Sussex Natural History Society.

Thursday, August 15.—*Objects Illustrative of Marine Life.*

Algæ—Sea-weeds, &c., fructification.
Diatomaceæ—Living and prepared valves.
Rhizopoda—Foraminifera—porcellanous, arenaceous, &c.
    Polycystina—recent and fossil.
    Spongiadæ—Calcareous, silicious, &c.
        Spicules, gemmules, skeleton, &c.

HYDROZOA—Corynne, sertularia, acalepha, &c.
ACTINOZOA—Alcyonium, gorgonia, helianthoida, &c.
ECHINODERMATA—Star fishes, ophiocoma, spatangus, comatula, cidaris.
  Echinus (shell, spines, ambulacra), &c.
  Holothuria, synapta, &c.
POLYZOA—Lepralia, flustra, halodactylus, bugula, avicularia, &c.
  Ascidia, salpidæ, &c.
MOLLUSCA—Shells, palates, cilia, eyes (cuttle), &c.
ANNELIDA—Serpula, aphrodita, &c.
CRUSTACEA—Zoea of crab; eye, shell, &c., of shrimp, prawn, lobster, &c.
  Pycnogonidæ, caprella, balanus, &c.
PISCES—Teeth, scales, dermal skeleton, &c.
REPTILIA—Turtle, &c.
MAMMALS—Teeth, bone, hair, whalebone, &c.

Tuesday, August 20.—*Objects of General Interest, either from their Novelty or Scientific Value.*

At both soirées full opportunity will be given for the display and inspection of microscopes and other optical instruments, with their latest improvements and additions.

BRIGHTON, *July*, 1872.

And asked the co-operation of the different country and metropolitan societies, as well as microscopists throughout the county, and the principal London makers. Promises of help came in very freely; but although the rooms in which the soirées were held included the Dome, the Royal Exchange, and the New Library and Picture Gallery, the space at their disposal did not admit of more than about eighty microscopes, allowing sufficient room to each to prevent crowding.

The arrangements made by the committee admitted the placing the members of societies at one long table, capable of accommodating fifty microscopes, and giving a similar table at another part of the Corn Exchange to the opticians; this enabled the Society to carry out the programme as arranged by the committee.

A number of living objects had been collected and dredged by Dr. Badcock, and other gentlemen; but to guard against accidents some 300 objects, illustrative of marine botany and zoology, were provided by Drs. Ormerod, F.R.S., and Hallifax; Messrs. T. Curties, Hennah, and Wonfor; classified in accordance with the memorandum issued by the Society.

The different societies which co-operated with the Society, and were represented by deputations, were the Royal Microscopical Society, the Quekett Club, the Lewes Natural History Society, the Croydon Club, the Eastbourne Natural History Society, the West Brighton Club, and the Brighton Medico-Chirurgical Society; while the gentlemen who exhibited were, Drs. Badcock, Carpenter, F.R.S. (the President of the Association), Hall, Hallifax, Kebbell, Ormerod, F.R.S., and Sim; General Worster; Captains Marshall Hall, and Noble; Revs. J. H. Cross and W. Payne; Messrs. Ardley, Aylen, S. Burrows, T. Cooper, T. Curties, J. Dennant, T. Glaisyer, R.

Glaisyer, Gwatkin, Gwatkin, jun., Hillman, Histed, T. H. Hennah, McIntire, Mitchell, Nash, W. D. Savage, F. E. Sawyer, J. Sewell, Shaft, C. P. Smith, W. H. Smith, Tatham, Turner, Unwin, Upton, C. A. Wells, Williams, T. W. Wonfor, and W. Wonfor, jun. About a dozen microscopes, belonging to different members of the Society, were placed at the disposal of Dr. Carpenter, for the purpose of exhibiting his deep-sea dredgings.

At the second soirée many very choice and interesting objects, including several living objects, were exhibited by the same gentlemen.

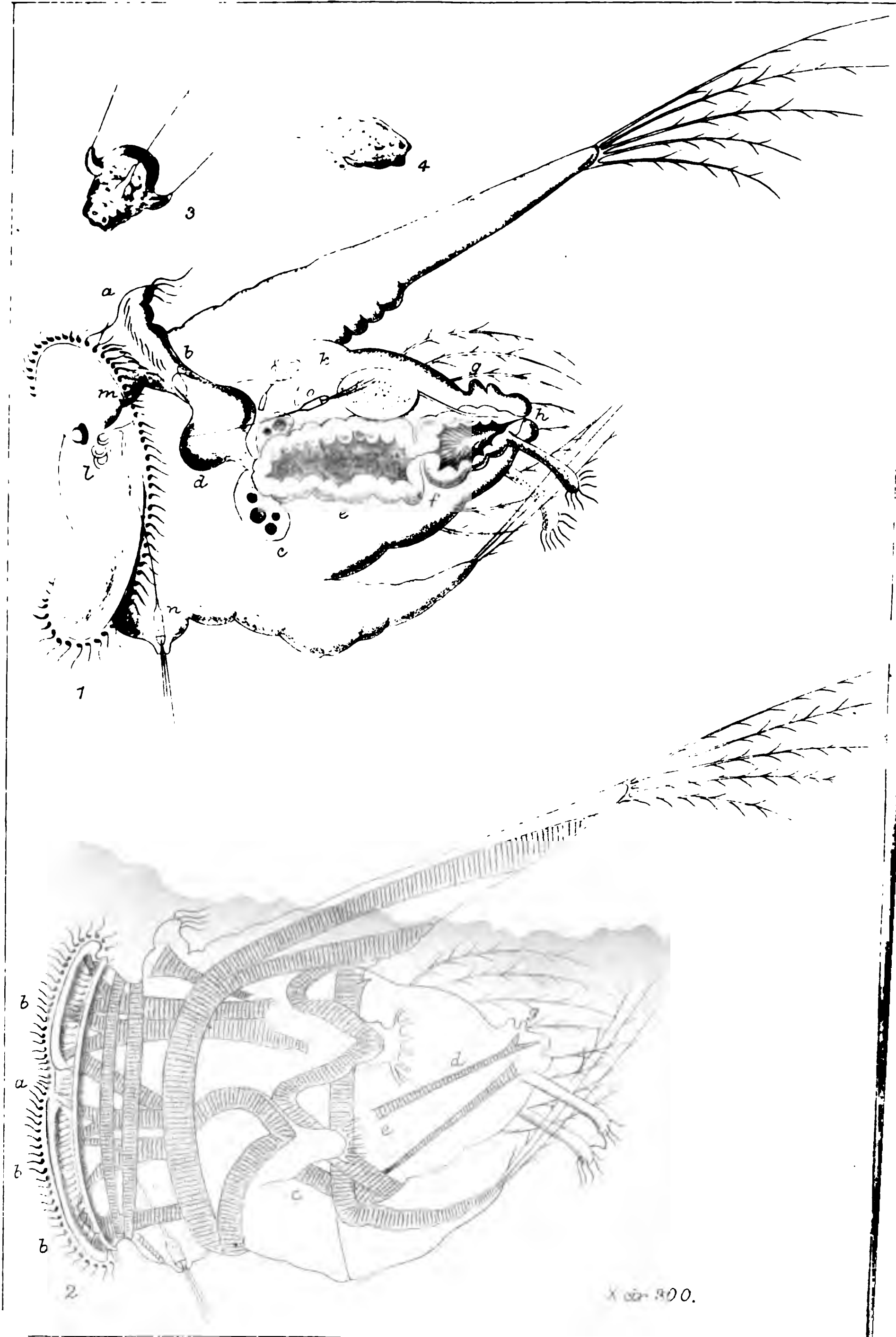
The opticians who responded to the Society's invitation and exhibited microscopes, were Messrs. Ross, who exhibited their new patent $\frac{1}{15}$th and $\frac{1}{10}$th objectives, and their tank microscope; Murray and Heath, who exhibited Dr. Gladstone's crystals; Ladd, who exhibited spectroscope, &c.; Horne and Thornthwaite; How, who exhibited hand and class microscopes; Swift; Moginie, who also exhibited revolving stereoscope with very beautiful slides; C. Baker, who placed several microscopes and lamps, and a collection of objects at the Society's disposal; Norman, who exhibited three large frames of very choice objects; and Wheeler, who exhibited book-folding cases containing 1000 objects.

The botanical display was arranged in three upstairs rooms of the new museum and library: two rooms were filled with living plants obtainable in the county during the month of August, and to the number of nearly 500 species were arranged in two tiers in long, narrow boxes filled with damp sand; and as this part was to be kept open not only during the Association's visit, but for ten days afterwards, the plants which withered were replaced from day to day by fresh ones.

In the third room dried plants were exhibited in three rows on the walls; while mosses, lichens, &c., Sowerby's 'English Botany,' 'Nature Printed Sea-weeds,' and other botanical works from the Society's library lay on tables. The management of the living flora was undertaken by Mr. Benjamin Lomax, and of the dried plants by Mr. C. P. Smith, who admirably carried out the views of the committee.

The following ladies and gentlemen contributed to the living flora:—Miss A. Woodhouse (Eastbourne) and Robinson (Saddlescombe), Mrs. Hodgson, the Rev. — Brewer (Worthing), Messrs. Griffith, Boys, Hadlow, T. H. Hennah, A. Bigge, H. J. H. Nicholls, F. Merrifield, W. Mitten (Hurst), H. Willett, G. D. Roper (Eastbourne), Nourse, Hall, G. D. Sawyer, W. H. Smith, C. P. Smith, G. Davies, B. Lomax, Helmsley (Hassock's Gate), and C. Hill (Rockhurst).

Among the exhibitors of dried plants were Miss Hall (Eastbourne), who lent a fine collection of flowering plants; Mr. W. Mitten, mosses and hepaticæ, all Sussex, including several unique species; Mr. C. P. Smith, Sussex grasses, and marine and fresh-water algæ; Mr. G. Davies, Sussex lichens, including the rare *V. Garovaglii* and *V. microcarpa;* Mrs. Hodgson, Brighton marine algæ; Mr. R. C. Roper, leaf fungi; Mr. Clarke, Mayor of Saffron Walden, new British plants; and Miss Turner, New Zealand plants.



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Pedalion mi

THE
MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1872.

I.—*Is Pedalion a Rotifer?* By C. T. Hudson, LL.D.

PLATE XXXIII.

Is Pedalion a Rotifer? To this I may reply, "What is a Rotifer?" a question as difficult to answer as Sir Robert Peel's famous query, "What is a pound?" Few things are so difficult to construct as a good definition, and there are few better ways of tripping up a troublesome antagonist than that of suddenly calling upon him to define some term which he is using. He must be a very capable man who can at a moment's notice so reply, that his definition may not be torn to bits almost before it has left his lips.

It is easy enough to define the typical form of a natural group of animals, or even to include in the definition forms that must be placed pretty near the central one; but, in the ambitious search for a definition which shall include many families, as we get further away from the typical form, we find that one by one all the positive statements are disappearing from our definition, till at last we have nothing left but the mere shuck of a proposition, shelled of everything worth the stating. Take the definition of one of the zoological manuals:—"Rotifers are microscopic animals, contractile, with vibratile cilia at the anterior part of the body, which by their motion often resemble a wheel moving rapidly. Intestine distinct; terminated at one extremity by a mouth, at another by an anus; generation oviparous, sometimes viviparous."

Now there are rotifers that have no "vibratile cilia at the anterior part of their bodies," others that have no intestine or anus, so that including these the above definition must shrink down into "Rotifers are very small animals that lay eggs."

That it is not an easy matter to answer off-hand the question "What is a rotifer?" may be readily seen by turning to one of our latest text-books, where no less than two handsome octavo pages are taken up in describing the characteristics of the class. I take for granted that the author considers the whole of this necessary in replying to the question, "What is a rotifer?" for if the first two sentences are to be considered as constituting the definition of the term "Rotifera," then the definition will collapse as completely as the former one under the weight of various aberrant (but clearly rotiferous) forms. Let us see: rotifers are "Vermes with a retractile ciliated disk at the anterior extremity of their bodies which are ordinarily microscopic in size, though they may attain as large a size as $\frac{1}{36}$th of an inch in length. They are usually more or less plainly annulated externally, but they never are divided internally into compartments by any transverse septa."

Now it is not long ago that I read an account of a discovery in Germany of a rotifer without any ciliated disk at all; this, if true, disposes of nearly the whole of the first sentence. As to the second sentence, the first part excludes the whole of the Brachionæa; and the second excludes the Floscules. Here again, then, the definition would shrink down into "Rotifers are small worms"; a statement which I trust that *Pedalion* will effectually dispose of.

Happily *Pedalion*, in all but its six limbs, too closely resembles such forms as *Brachionus pala* and *Triarthra longiseta*, to compel us to discuss such a knotty question as that of how much of a rotifer's form and structure an animal may lose and yet remain a rotifer. In Plate XXXIII., Fig. 1, I have taken the liberty of removing two of *Pedalion's* limbs as well as all its muscles, so as to show its internal structure. Its large trochal disk, though of a pattern peculiar to itself, is still constructed on the same general plan, and with precisely the same object as those of *Rotifer vulgaris* and *Melicerta*. An imperforate oval disk bears on its edge large cilia, which by merely moving up and down, draw the water into currents setting right against the face of the disk. These currents on striking the disk are forced along the face of it outwards in all directions between the large cilia. Below the edge of the trochal disk lies a circular groove which surrounds the whole animal at its neck, and commences and terminates at its mouth. The outer edge of the groove is densely ciliated, and the cilia are very fine and set very closely together. When, then, the currents due to the larger cilia drive their floating atoms over the edge of the trochal disk, the smaller cilia lash the atoms back into the groove and hurry them along on both sides of the animal towards the mouth. This process can be readily watched in *Melicerta* or *Limnias*, and not quite so easily in *Œcistes*, and has been often described: in *Pedalion*, too, it can be distinctly seen by those who have either the patience to watch for a favourable opportunity, or the good luck to catch the animal resting at the proper moment.

Pedalion's trochal disk has a curious thin portion at each side (Fig. 2, *a*), bounded by branches of two longitudinal muscles (Fig. 2, *b*), and for the purpose I believe of permitting the disk to be neatly folded when acted upon by the retractor muscles. Of these *Pedalion* possesses no less than six on each side (Fig. 2, *b*), as well as one long one (Fig. 2, *c*), which is attached to a point in the mid-dorsal line just under the dorsal limb, and stretches obliquely thence to the trochal disk, where it is fastened symmetrically by two branches.

To return to the atoms which have been driven along the ciliated groove till they reach the buccal funnel (Fig. 1, *a*). This latter resembles that of *Triarthra;* it is densely ciliated, and leads directly to the mastax. The mastax is like that of *Limnias ceratophylli;* and the entrance to it from the buccal funnel is guarded as in *Brachionus Mülleri* (and other species) and in *Floscularia campanulata*, by two chitinous lips (Fig. 1, *b*), if I may so term them, which are often raised to secure some desirable morsel. The food after being comminuted in the mastax enters a short œsophagus, and passes thence into a nearly cylindrical stomach. The walls of the stomach are very thick and elastic. In a dying specimen I have seen the food expelled and the walls close right in upon themselves. In general the stomach is greatly distended with an orange-coloured food, and gets sadly in the way of anyone who wishes to make a paper for the Microscopical Journal. At the anterior end of the stomach are the usual pair of gastric glands (*c*); less spherical in form, it is true, than they are in many rotifers, but less aberrant than those of *Pterodina patina* or of *Brachionus Mülleri.* I am pretty sure, too, that I saw now and then two other small glandular-looking bodies on the œsophagus, similar to those of *Euchlanis deflexa*, figured by Gosse in his paper in the 'Philosophical Transactions' on the manducatory organs of the rotifers; plate xvi., fig. 12, *n*.

From the stomach the food passes into a ciliated intestine (*f*), a broad short chamber with even thicker walls than those of the stomach, and with much coarser cilia. Although the division between the stomach and intestine is perhaps more marked in *Pedalion* than in the majority of rotifers, still it exists to some degree in all that I have seen except in *Asplanchna priodonta;* though in some it is little more than a temporary folding-in of the lower third of the alimentary canal.

The fæcal matter is expelled at the anus (*h*). For some time I imagined that the anus lay at the point (*g*) in the ventral surface, where is seen a curious projection. It is true that this would be a unique position—but then a rotifer with six legs may be considered as fairly entitled to have it in a unique position. Moreover, both the ciliated intestine and the passage of the anus are often so completely closed that no one would suspect there was an opening. Add to this that the viscera do not always lie in the handy position in which I have drawn them; but so fill the posterior extremity that there would be no apparent difficulty in the aperture's being at (*g*). After I had seen the fæces expelled and had ascertained the true position of the anus, I was still puzzled with the spot (*g*). There was an appearance of corrugated skin overlying the ventral surface round about it, which made me fancy at first that the particular animal I was looking at was unhealthy, or had suffered some accident; and that the projection at (*g*) was not normal. However, this explanation would not do, for almost every specimen had the same appearance. It is only quite lately that I arrived (as I believe) at the right conclusion. I had the good fortune to find a *Pedalion* with an almost empty stomach and intestine, with a skin brilliantly transparent and quite free from the various parasites that often mar its beauty. The first glance at this gem showed me a muscle which had before escaped me, and which I have described above, and drawn in Fig. 2, *c*. The second showed me another new pair of muscles (Fig. 2, *d*), each with one branch attached to the posterior extremity and the other at (*g*), the broad attachment being at (*e*), and the effect of the whole arrangement to give a sideways pull at (*g*). It occurred to me at once that this must be the aperture of the oviduct, and that the corrugated skin surrounding it is due to the distension caused by the passage of the large eggs. It is true that such an aperture is almost an unheard-of luxury in a rotifer, and that *Asplanchna*, I think, alone possesses one; and in its case the aperture is a necessity, not a luxury; for without it this unhappy rotifer would have to expel its young as well as its fæces from its mouth. But the ovarian apparatus in *Asplanchna* is precisely similar, and I have seen it used; I have seen the aperture opened by its proper muscle, and the young *Asplanchna* expelled. The obvious way to determine the point would be to watch *Pedalion* laying an egg; but although I have had scores of specimens under observation I have never seen an egg laid. I watched in vain one specimen, in which there was an egg apparently nearly mature, for upwards of two hours; and then I desisted, for eyesight is more precious than rotifers. It is, however, of little consequence, for rotifers are so constantly laying eggs that some observer is sure to see the process next year.

Pedalion lays both female and male eggs, and, with respect to them, resembles the majority of the rotifers in the following points; *viz.* eggs of different sexes are not found on the same individual; virgin females lay female eggs during the greater part of the year; male eggs are half the size of female ones, and are carried in clusters of often a score at a time; and, finally, the males are small abortions without any organs for procuring or digesting food, appear at only one period of the year, and live a very short time—

sometimes not an hour. They are shown in Figs. 3 and 4, of their proper relative size, swimming round their mother.

Starting from somewhere on or under the intestine, and at its posterior extremity, are the two usual convoluted tubes (Fig. 1, *k*). Each of these has one ordinary bunch of loops at (*k*), with two vibratile tags just above it, and another bunch at (*l*), close under the eye, with a third vibratile tag close adjoining.

This system of tubes and tags is most difficult to see in *Pedalion;* not only are the lower portions of the tubes unusually transparent and unusually free from loops and tags, but the upper portions, where alone are the loops and tags, are hidden generally behind a stout striated muscle running from near the eye to the outer limb, and the muscle itself has frequently in front of it some portion of the curved and folded surfaces of the same limb.

Under these circumstances, it is needless to say that the tags and loops are not to be seen; and these unfavourable conditions are often aggravated by the stomach's being so distended as to form a coarse shifting yellow background to every portion of the field of view. It was not till I had nearly exhausted my stock of rotifers that I caught sight of the vibratile tags again this year; but since then I have repeatedly traced the tubes from end to end on several specimens.

Pedalion has a nerve mass (Fig. 1, *m*) closely applied to the buccal funnel, with two eyes (clear spheres on plates of red pigment) seated on it, and with nerve threads connecting it to the ganglionic intumescences of the dorsal and shoulder antennæ (*n*, *o*). In all these respects it closely resembles *Triarthra;* and a somewhat similar nervous system is visible in most of the free-swimming rotifers. There are also two delicate threads, commencing at the aperture of the oviduct, but whose other ends I have been unable to find.

I may here point out a curious and persistent difference between the free-swimming rotifers and the tube-forming ones. Most of these latter—*Melicerta*, *Limnias*, and *Œcistes*, for example—have their antennæ on the ventral surface, just below the ciliated chin, which underlies the mouth: but the free-swimmers have their antennæ on the dorsal surface, the normal number appearing to be three; one on the mid-dorsal line at the neck, and either two at the sides, as in *Triarthra*, *Hydatina*, and *Asplanchna*, or two at the posterior extremity, as in *Synchæta*.

The ciliated projections at the posterior extremity of *Pedalion* seem at first sight as abnormal as its six limbs: but as they exude a viscid secretion I have little doubt that they represent the "pincers" of *Brachionus*, *Synchæta*, and others, none of which "pinch," but all of which secrete a web by which the animal moves itself at will. In *Brachionus* the "pincers" are at the end of the tail of the pseudopodium; in *Pterodina* the pincers disappear and the tail ends in a ciliated cup; while in *Pedalion* the tail disappears and the pincers are ciliated and set widely apart.

These projections are of various shapes and most varied length, and Mr. Slack called my attention to the fact that in some specimens the cilia were invisible. I did not find this to be often the case in my specimens; but I did meet with one or two in which I could not see the cilia. Possibly—as suggested by Mr. Slack—the cilia may at times cease to vibrate, and then lie close to the organ which carries them; and possibly they may be shed by an animal which is not quite healthy. Indeed, I found one pair, on one of which there was only one cilium feebly lashing the water, and on the other but two stumps: there would be nothing unusual in this, as I have frequently seen *Floscularia campanulata* shake off the long setæ which fringe its disk, and have seen the pieces float away. The cilia, too, on the trochal disk of dying rotifers come off on the slightest disturbance; and *Stephanoceros*, when unhealthy, may be seen to throw off considerable portions of its fringed arms.

Pedalion's internal structure, as detailed above, closely resembles that of any one of the typical rotifers. If this be admitted, it must, I think, also be conceded that through some of its forms the class Rotifera is linked to the Crustacea rather than to the Annulata.

Who, looking at *Pedalion's* pairs of jointed limbs worked by appropriate muscles, would think of placing it among the worms?

Gosse long ago insisted that the Rotifers must be ranked among the Arthropoda, adducing in evidence of this the approach of the manducatory apparatus to that of an insect; the curious condition of the males, resembling that of the males of the Cirripedia; and the presence of true condyles in the joints of such rotifers as *Dinocharis* and *Polyarthra*. To these three weighty reasons may now be added *Pedalion's* three pairs of legs; and the effect of the whole is, I think, irresistible.

In Dr. Rolleston's 'Forms of Animal Life' it is stated that the sub-kingdom VERMES consists of animals "bilaterally symmetrical, very various in shape and other external characters, but agreeing in the absence of heteronomy from their post-cephalic regions, in not possessing hollow segmented limbs, and in having their locomotor muscles closely connected with their integumentary system, not only on the ventral, but also on the dorsal and lateral aspect of their body walls."

Now, if the above is to be accepted as the definition of the sub-kingdom VERMES, then surely it excludes *Pedalion*: for *Pedalion* is strikingly heteronomous both as to its external shape and internal organs, possesses six hollow limbs, and though its muscles are necessarily attached to the integument, yet they cannot be said to be closely connected with the integumentary system; the majority of them touch the integument at their extremities only, and moreover lie often well away from it, and may be seen now and then to hang quite slack, or even to form wide loops; and *one, viz.* that figured at Fig. 2, *c*, actually traverses two-thirds of the innermost portions of the animal's body, passing from the mid-dorsal line to the face of the trochal disk, to which it is attached by a double head.

Nor is *Pedalion* the solitary rotifer which refuses to submit to this definition. The great majority of rotifers are fairly heteronomous, even as to external shape, and undoubtedly so when both external shape and internal organs are taken together. No transverse section, however thin, of any rotifer that I am acquainted with, repeats itself in any portion of the body; while *the* distinctive feature of a worm is, that transverse sections of definite thickness do repeat themselves so remarkably that there seems no reason why a worm might not be 6 feet or 6 yards long, instead of 6 inches. Besides, in most of the rotifers that I have studied—for instance, in *Brachionus*, *Synchæta*, *Pterodina*, *Asplanchna*, *Rhinops*, *Triarthra*, *Polyarthra*, and others—the main muscles, *viz.* those for retracting the disk, are attached to the integuments at their extremities only, and freely traverse the perivisceral space, at a good distance from the integument.

As to the possession of "hollow segmented limbs," there the rotifers have a weakish case; still it has not been left to *Pedalion* alone to fight the battle for his less-gifted brethren. *Synchæta* has two hollow rudimentary limbs, each worked by two long powerful muscles, attached to spots inside of them, as may be seen in my drawings in the fourth volume of this Journal; and by placing these powerfully ciliated limbs at different angles to its body, is able to perform many of its curious antics. *Triarthra* and *Polyarthra*, too, have processes that might pass muster among the Greenwich Pensioners; but I am afraid they do not help the case much: *Polyarthra*, however, has distinct condyles (as Gosse has shown), on which its curious processes work; and *Dinocharis* has equally distinct joints.

Doubtless all general statements in Natural History must be taken with wide allowance; definitions, classifications, systems are, after all, but useful pegs to hang our knowledge on; and it must often be a matter of taste almost, how some odd creature is to be ranked and ticketed. If it is desired to establish its genealogical relation to other animals, then indeed the matter ceases to be one of taste, and becomes one of intense interest, and one which may well give rise to warm controversy; but such discussions I must leave to those very few who have the requisite knowledge and capacity. I would only venture to hope that, as my readers weigh the various reasons for or against the promotion of the rotifers, they will admit that, when *Pedalion's* six limbs are thrown into the scale, the beam decidedly inclines towards the Arthropoda.

EXPLANATION OF PLATE XXXIII.

FIG. 1.—Side view of the female of *Pedalion mira*, showing the internal organs. The two side limbs have been removed, as well as all the muscles. *a*, buccal funnel. *b*, lips. *c*, gastric glands. *d*, mastax. *e*, stomach. *f*, intestine. *g*, aperture of oviduct. *h*, anus. *k*, loop of depuratory tube, with two vibratile tags close adjoining. *l*, another loop of depuratory tube, with one tag. *m*, nerve mass. *n*, dorsal antenna, with nerve thread. *o*, shoulder antenna, with nerve thread.

FIG. 2.—The same view of the same to show the muscular system: the plumes of the side limbs and all the internal organs removed. *a*, thinned portion of trochal disk. *b*, *b*, *b*, muscles for retracting the disk. *c*, very long ditto, traversing the centre of the body and fastened by two branches to the centre of the trochal disk. *d*, *e*, muscle for opening oviduct. *g*, aperture of oviduct.

For full description and figures of the rest of the muscles, see the October number of the 'Quarterly Microscopical Journal' for 1872.

The figures, however, given in that number, as well as those accompanying this paper, are rather diagrams than drawings. For drawings giving the best representation of *Pedalion's* appearance, see the sixth volume of the 'Monthly Microscopical Journal,' page 121.

II.—*On the Structure and Development of the Crow's Skull.*

By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *October* 2, 1872.)

Plates XXXIV., XXXV., and XXXVI.

Introductory Remarks.

My first materials for working out this type of skull were obtained for me by our esteemed member, W. H. Ince, Esq. Other specimens came to me from friends in different parts of the country, no few of these having been laid under contribution for hatched and unhatched chicks of various birds. The Crow is the great subrational chief of the whole kingdom of the Birds; he has the largest brain; the most wit and wisdom; albeit, at times the cruellest of the birds of wing. He, or at least some of the secondary chieftains of his tribe, can speak with man's voice, and, full of drollery and satire, make mockery of all and every of the many voices that are in the world.

Half the known breeds that are, owe allegiance to the kingly Crow: of a full and literal *myriad* of feathered species, a good half are ennobled by Mr. Huxley's title, namely, that high-sounding and yet hoarse name—"Coracomorphæ."* This term — the *Crow-form*—is not to be taken as interchangeable with the name of the Crow family—"Corvidæ"—but as the expression of a sweeping generalization which gathers together from the very ends of the earth all those birds that have the same morphological characters as the Crow. Even birds that are to all intents and purposes *Crows* have to be broken up into many "sub-families,"—there are the *Northern* and the *Southern* Crows; the Starlings, Choughs, Nutcrackers, Birds of Paradise, and a host of others; all Crows, not, strictly speaking, "Corvidæ," but "Sturnidæ," "Paradisæidæ," and the like.

Now the term "Coracomorphæ" may be used to express such a general physiognomy as would make an ordinary observer say of a bird, "That form reminds me of a Crow:" such a remark would not be made concerning a Grouse, a Plover, or a Cormorant.

But there underlies the visible form and fashion and colour of a living bird some modification of its plan of structure which is always the correlate of certain outward characters; and an anatomist familiar with the construction of the various types seldom makes a wrong guess as to what morphological group he will have to refer any new bird, when he has taken a good survey of its outward form. Professor Huxley, in the paper referred to, has seized hold of the great controlling portion of a bird's structure, namely, its head, and especially its *face*: by the structure of the face, which must have all things else in harmony with it, he has sought to give the birds a true morphological grouping. Such a system of taxonomy has great and evident value; but in the present state of our knowledge the physiognomical groups do not admit of simple superposition on the morphological groups. In some cases they fit accurately; in others they come short in one place and overlap in another; and thus the zoologist has to "drive two-in-hand" through the various ornithological territories; and as the various regions in a geological map cannot be made to correspond with political or with ethnological divisions, so it is in our present attempts at making the two taxonomic modes of *bird-survey* agree.

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* See "On the Classification of Birds," 'Proc. Zool. Soc.,' 1867, pp. 415–472.

If the palate of a Fowl be examined, it will be seen to be deeply cleft; if that of a Goose, the cleft will be found to be closed in front: the palate of a *Podargus* (Giant Goatsucker) has its two sides bound together by two bony plates—the hard palate. The cleft in the Fowl and Goose is divided into two, by the "vomer," or ploughshare-bone, which lies at the middle.

In the Fowl neither the upper maxillaries nor the palatines meet at the mid-line below; in the Goose the maxillaries do thus meet and combine; in *Podargus* both the maxillaries and palatines coalesce, as in Man and the other Mammalia.

For the perfectly cloven palate of the Fowl, Professor Huxley has coined the term "Schizognathous"; for the half-closed palate of the Goose, as well as for the well-floored palate of the *Podargus*, the same *Mint* has yielded the term "Desmognathous."

But the *Crow-form* stands in need of another name for its type of palate than either of these; and, as the Common Sparrow well displays this third kind of mouth-roof, our bold group-maker calls it the "Ægithognathous" palate. Now I could have wished that either this bird or the Crow had given both names, that is to say, to the general type and to the palate; then we should have had the Coracomorphous birds with Coracognathous faces, or the Ægithomorphous birds with Ægithognathous faces.

Nevertheless, the two groups are almost at one, in reality, and it does not weigh a feather with me how the thing is put, if so be the student will get his mind clear upon the subject.

The cleft in the Sparrow or Crow's face is stopped-up in front, not by coalescence of the maxillary bones, which are similar to those of a Fowl, but by having a double vomer, which unites at the mid-line in front, where it is broad, emarginate, and winged.

These antero-lateral wings are supplied on each side with an additional bone. The two bones are grafted upon the cartilaginous nasal walls and their long scrolls—the "ali-nasal" or "*anterior* (not *inferior*) turbinals."

This metamorphic fusion of parts, essentially distinct in morphological origin, is very remarkable; and to add to the mystery, the vomerine moieties are each of them formed in a distinct plate of cartilage, which, as a rule, does not exist in other birds.

The "Coracomorphæ"—five thousand known species there are of these—are characterized by this structure, at least as far as I can find, for I have worked out this part in scores of types,* not only of the natives of the Old World, but to an equal or even greater extent in South American, Malaysian, and Australian forms—the feathered denizens of the "Notogæa," or Southern World.

The Swifts ("Cypselus") have this structure as completely as their Passerine relations the Swallows. *Just above* the Ostrich order, the *Hemipods* ("Turnicomorphæ") show this structure on a very large scale, but the bony substance is only strongly tied to the cartilage.

Leaving out the Tinamous, which are merely little Ostriches struggling to become of the winged order, there are certain *climbing* birds which refuse to conform to my friend's Greek rules, "and he whose heart is as the heart of a lion, did utterly melt" in presence of this difficulty; these birds, the Woodpeckers, retaining the strongest marks of any of a reptilian parentage, I beg to call the "Saurognathæ."

Differences of so remarkable a kind existing in the fore-face of the great groups of birds, and correlated with such exquisite specliazations as we find in the Woodpeckers on one hand, and the Singing-birds on the other—to say nothing of other groups, each member of which is a marvel of adaptive modification—such peculiarities as these may well decoy the student into these enchanting zoological territories.

On the Development of the Skull and Face of the Genus Corvus.

My youngest Crow-embryo is the Hooded Crow (*C. cornix*) at about the sixth day of incubation. The natural size of its head is shown in Plate XXXIV., Fig. 1. A pâlatal view of the skull (Fig. 2) at this stage shows that the first "præ-oral arch" (*trabeculæ tr.*) has coalesced throughout nearly its entire length, the posterior part (apices of the rods) alone being distinct. These apices diverge round the pituitary body (*py.*), and then coalesce with the "investing mass" (*i. v.*) of the notochord (*n. c.*). The distal ends, besides coalescing by a continuation forwards of the median commissure, have sent forwards an azygous rostrum, the "pre-nasal cartilage"; this single cartilage answers to the double cartilages in which the mandible terminates.

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* Through the kindness of my friends, Osbert Salvin, Esq., and Mr. W. J. Williams, Assist. Sec. Zool. Soc.

On each side, in the indifferent tissue that covers this rod *above*, the præ-maxillaries are found, as small oval patches of osteoblasts (Fig. 3, *p. n.*, *p. x.*). The cartilaginous cranium is already well formed, and the auditory sacs (*au.*) with their enclosed otoliths (*ot.*) are well seen in this delicate skull when examined as a transparent object. The pituitary space (*py.*) is still freely continuous with the space in which the notochord (*n. c.*) lies—"the posterior basi-cranial fontanelle"—but the halves of the "investing mass" (*i. v.*) have united on each side with the auditory mass, and also with each other behind; where they have formed the bilobate occipital condyle (*o. c.*). From the investing mass on each side, a little in front of the condyle, there is a crest of cartilage, the "ex-occipital" (*e. o.*), which is growing towards its fellow in the "super-occipital" region (*s. o.*), and is becoming coalesced to the hinder margin of the auditory sac.

A side view of the cranio-facial axis, from the "sella turceca" forwards, is shown in Fig. 4; it takes in the whole trabecular bar. Here we see how large the terminal cartilage is, notwithstanding its temporary character, and also that the trabeculæ have sent upwards a crest of cartilage which is continuous with the keel growing from the cranial *floor* and nasal *roof* from the optic foramen (2) to the commencement of the pre-nasal rostrum (*p. n.*). Not only has continuous metamorphosis of the mother-cells into cartilage made these various morphological parts one, but the roof of the nasal sacs and the floor of the hemispheres have undergone the same change. Between the eye and nose the nasal wall has become a large uniform plate of cartilage; this is the "pars plana" (*p. p.*) or "ecto-ethmoid." A transverse section through this region (Fig. 5) shows the relation of the "ecto-" to the "meso-ethmoid." In another section (Fig. 6) the septum nasi is cut through, and the ali-septal cartilages are seen each to give off an elegant scroll with two turns; this is the equivalent of the human "inferior turbinal," although its position is in *front* of and not *below* the true olfactory region. A section made between the eye-balls (Fig. 7) shows the cartilaginous continuity of the two trabeculæ; their ascending crests, the anterior sphenoidal keel (præ-sphenoid), and the "orbito-sphenoidal laminæ."

Most of the investing bones can now be seen. I have already spoken of the præ-maxillaries, and others will be better shown in an embryo more advanced, but four out of five of the mandibular splints can now be well observed. On the inner side of the "articulo-Meckelian rod" (Fig. 8, *cr.*) the "coronoid" is seen at the top; and the "angulare" (*ang.*) on the inner side, and part of the "surangulare" (*s. ag.*) below. On the outer side (Fig. 9) the whole of the large "surangulare" is shown, as also, much further forwards, the "dentary" (*d.*): the "splenial" (*sp.*) is shown in a more advanced stage.

The Monthly Microscopical Journal. Novr 1. 1872. Pl. XXXIV.

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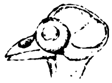

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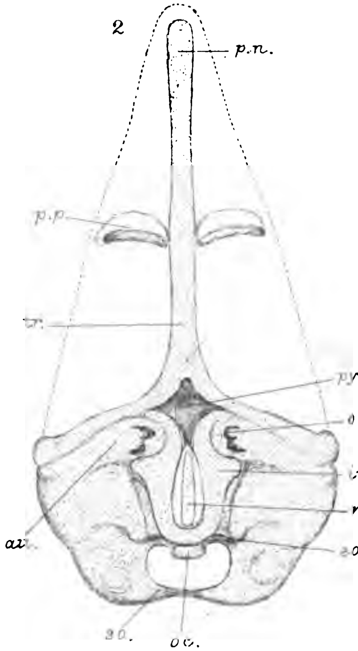

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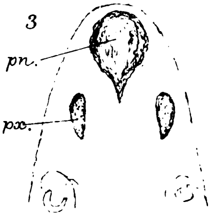

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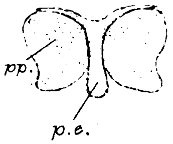

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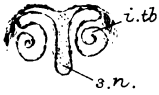

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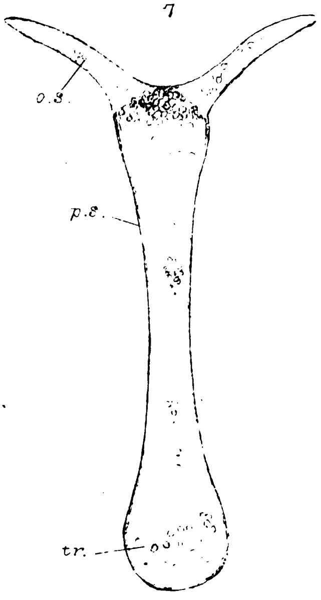

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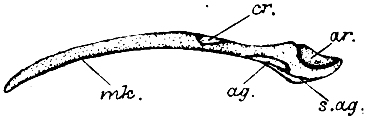

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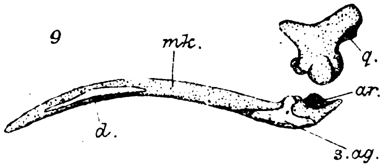

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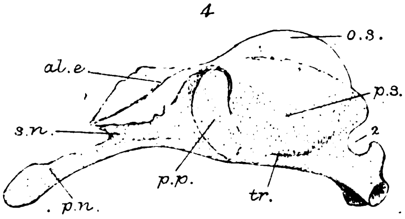

W.K.P. del ad nat. G. West on Stone.

W. West & Co. imp.

Development of Crow's Skull.

This more advanced stage has been obtained in an embryo of the Rook (*C. frugilegus*), two days older, or about the eighth or ninth day of incubation. The basal view of the skull (Plate XXXV., Fig. 1) shows several things not present, and others not figured in the earlier stage. The cartilaginous skull is well formed, and its *first* bony instalment is gained, namely, the "basi-occipital" (*b. o.*). This is formed as a delicate bony investment to the cranial part of the notochord, and then spreads laterally into the halves of the "investing mass" (*i. v.*). The whole occipital cartilage is now well seen with its condyloid foramen (9), and the passages for the "vagus" nerve (8). The condyle (*o. c.*) is more typically ornithic, being nearly hemispherical; the great tympanic alæ also grow widely from the ex-occipital region (*e. o.*), which is not ossified as yet. The super-occipital cartilage (*s. o.*) is a thin commissure uniting the huge ear sacs (*au*). This is still better seen in Fig. 2, where the anterior, horizontal, and posterior semicircular canals have stretched the auditory cartilaginous coat almost to bursting. The "ali-sphenoidal" cartilages grow up *directly* from the pituitary part of the "investing mass" (Fig. 1, *py. iv.*), but the space itself, embraced in front by the *top ends* of the trabeculæ, is membranous below. So much of the "basilar plate," or investing mass, as is not ossified by the notochordal bony sheath still forms a broad floor with a free front edge. The "posterior sphenoid" here borrows—*afterwards*, from the symmetrical ossifications formed in the thick sub-mucous cushion which is stuffed between the fauces and basis-cranii.

These bones, my "basi-temporals," are beautifully shown in this stage (Fig. 1, *b. t.*), as also the grooved bony style which yields hardening material to the fore-part of the basi-sphenoid; this is the "parasphenoid" (*pa. s.*). Further forwards, the cartilaginous base is seen between the palatines (*pa.*), and this becomes cleft, vertically, to form the hinge between the face and skull. The rest of the cartilage is the base of the nasal septum (*s. n.*), and the "præ-nasal rostrum" (*p. n.*). Besides the parasphenoid, the trabecular arch has the præ-maxillaries; these are very much increased in development since the last stage (Figs. 3, 4, 5, *p. x.*). Seen from above (Fig. 4), they already meet, nearly cover the "præ-nasal," and send their nasal processes to the ethmoid, above.

The vomerine splints were not well made out in this preparation, although I obscurely saw the division into two halves; but in a Brown Linnet at this stage, I found them to be formed as "endostoses," in an oval patch of *cartilage*. Above the trabecular region (Fig. 3) the "ali-nasals," "ecto-ethmoid," and "meso-ethmoids," are shown as increasing in size and becoming perfect in form, and between the ethmo-presphenoidal plate (*eth. p. s.*) and the trabecular crest a thin longitudinal cleft has been formed, the membranous interorbital space (*i. o. s.*). The "nasals" (Figs. 3 and 4, *n.*), the frontals (*f.*), the parietals and squamosals (Figs. 2 and 3, *p. sq.*), are now well demonstrable; but the great "fontanelle" (*fo.*) occupies most of the cranial roof. The second pair of *præ-oral* facial bars are undergoing rapid development; at first delicate crescentic bars of soft mother-cartilage, or indifferent tissue, they are now partly *true* cartilage, but principally bone. Behind (= *above*) the pterygoids fit by a cup to a ball on the fore-face of the pier of the next arch, the quadrate, and then, converging to the mid-line, they send a "meso-pterygoid process" over the upper edge of each palatine. This *process* afterwards becomes a separate bone.

Where the palatines (*pa.*) are bowed out behind, they send outwards a triangular plate, which, not becoming infected by the bony matter of the main bar, develops rapidly into hyaline cartilage, to have, afterwards, its own endosteal centre—the "trans-palatine."

Two splints are seen on each side, belonging to the pterygo-palatine arch. These are the very small *ichthyic* maxillaries (*mx.*), and the slender "jugals" (*j.*) which clamp the quadrate without the intervention of a "quadrato-jugal," as in many birds. The retral process of the maxillary (maxillo-palatine of Huxley) can already be seen passing towards the mid-line, *above* the palatines.

The hyoid arch, well developed as to cartilage, but with no bony deposits, was not wholly worked out in this stage. It will be described in the next.

But the mandible is now very instructive. Seen on the inner side (Fig. 5), the dentary (*d.*) is seen to have surrounded all but the inner face of Meckel's cartilage (*mk.*) in front; farther backwards the most splint-like of the splints—the splenial (*sp.*)—is now evident, and the coronoid (*cr.*), the "angular" (*ag.*), and the surangular (*s. ag.*), are growing rapidly; the two latter are shown in relation to the articular cartilage (*ar.*) in Fig. 6. Ossification had commenced in the proximal portion of the rod. The articular region (*ar.*), which, as seen from below (Fig. 6), sends backwards the "posterior angular process," and inwards the "internal angular process." This latter is the otherwise-used symmorph of the "Manubrium Mallei" of Man.

My next stage is supplied by the ripe chick of a Carrion Crow (*C. corone*, Plate XXXV., Fig. 7, and Plate XXXVI., Figs. 1–6). The process of development during the last five or six days of incubation is very rapid, and indeed very rapid is the whole process of growth in these high, noble, hot-blooded birds. The study of their growth and metamorphosis, as compared, not only with that of a Reptile or Fish, but even with that of the young of the slow-growing "Præcoces," suggests the possibility of a compromise between the doctrine of *Development* and that of *Creation.*

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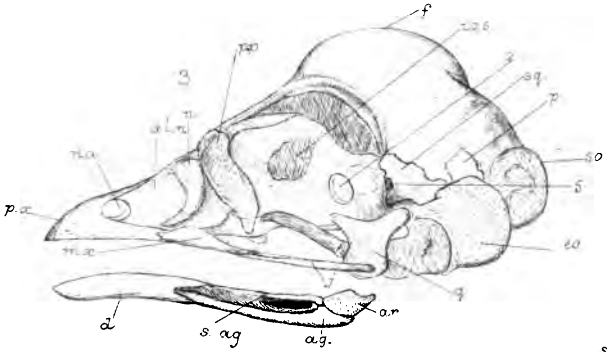

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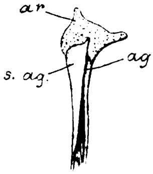

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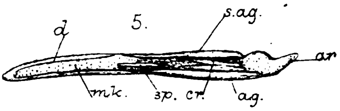

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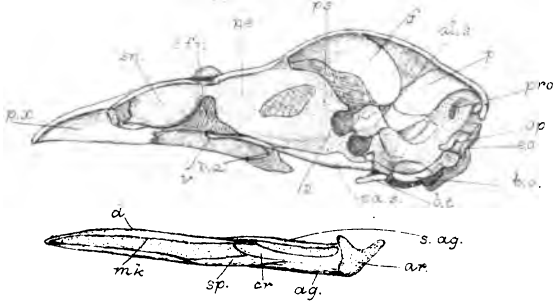

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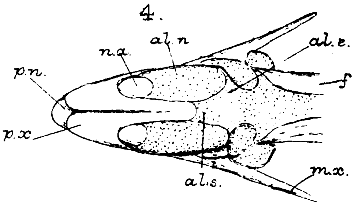

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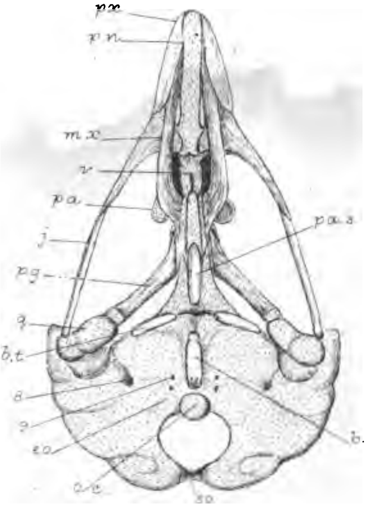

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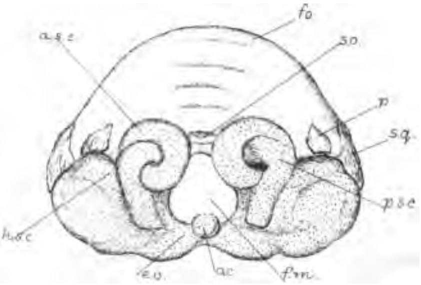


W.K.P. del. ad nat. G. West on Stone

W. West & Co

From the stored-up wealth of the egg things new and old have been required, and an architectural process has freely applied the *lime* which was ready at hand. This hardening of the walls has given us new bony centres, and has obliterated the distinction between others that were separate. Turning to the basal view, we see that the minute bone-sheath of the notochord has become a recognizable "basi-occipital" (Plate XXXVI., Fig. 1, *b. o.*); the "basi-temporals" (*b. t.*) have become one broad unbent-bow-shaped piece, and the parasphenoid, or "sphenoidal rostrum" (*r. bs.*), has got its *ichthyic* "temporal wings," has grafted itself upon the skull-base, and is being fused with the basi-temporal. The lateral occipital region has now its proper ossifications, the ex-occipitals (*e. o.*); and the upper part of the arch has begun to ossify by another pair of centres, the supra-occipitals (Plate XXXVI., Figs. 4 and 5, *s. o.*). On the roof (Fig. 3, *p.f.*), the parietals and frontals meet at the mid-line, and the nasals (*n.*) and præ-maxillaries (*p. x.*) have begun to acquire their permanent form. In the side view (Fig. 2), the squamosal (*sq.*) is seen flanking the roof-bones; the ali-sphenoid (*al. s.*) has a bilobate osseous centre, evidently by the fusion of two smaller ones. Attached to the large "ecto-ethmoidal" flap, or "pars plana" (*p. p.*), is a small rod-shaped lachrymal (*l.*); in this side view also the fish-like character of the maxillary (*mx.*) is well seen, as also the ectosteal patch on the quadrate (*q.*). Below, the pterygo-palatine arcade (Fig. 1, *pg. pa.*) has developed greatly, and the free cartilaginous "trans-palatine" flap has become typical in form. The mandible (Fig. 2, outer view; and Plate XXXV., Fig. 7, inner view) is now in a characteristic *reptilian* condition, with its one endosteal bone and its five parosteal investments. In the hyoid arch the shaft of the stapes (Fig. 2, *st.*), and the proximal pier of the "cornu major" (Fig. 6, *br.* 1), have each acquired an ectosteal sheath. The small cerato-hyals (*c. h.*), the "basi-uro-hyal" (*b. h.*, *u. h.*), as well as the distal part of the long rod (third post-oral), are still mere cartilage.

A longitudinal section (Plate XXXV., Fig. 7) shows much that is instructive. The exact relation of the roof-bones to the cranio-facial axis; the large ear-capsule, with its prootic (*pro.*) and opisthotic ossifications (*op.*)—the only centres I have found in this type; and the notch and fenestra that re-differentiate the trabeculæ.

The "interorbital fenestra" (*i. o. s.*) is suboval, and somewhat tilted backwards. The cranio-facial cleft (*c. f. c.*) has severed the thick trabecular bar and its crest, and is creeping upwards so as to divide the ethmoid (*eth.*) from the nasal septem (*s. n.*). The vomerine moiety (*v.*) is seen beneath the hinder half of this notch; and beneath it the palatine (*pa.*). The "rostrum," or parasphenoid, nearly touches the basi-temporal (*b. t.*); and partly ossifying the "sella turceca," has behind it the "spheno-occipital synchondrosis."

This is followed by the growing basi-occipital (*b. o.*); this by the condyle (*o. c.*), and this is surmounted by the "ex-" and "super-occipitals" (*e. o.*, *s. o.*).

My next stage is a nestling of the Carrion Crow (*C. corone*), one week old.

The most noteworthy thing in these further illustrations is the great increase of size, every tissue having undergone rapid interstitial growth; but the relative proportion of the various bones and tracts of membrane and cartilage is much the same as at the time of hatching.

Certain morphological changes of great importance have nevertheless occurred, for the "super-occipitals" (Plate XXXVII., Figs. 1–5, *s. o.*) have coalesced at the middle, and the "meso-ethmoid" (Figs. 2 and 4, *p. e.*) has acquired a bony centre.

The lachrymal (*l.*) is larger, is creeping up the fore-edge of the ecto-ethmoid (Fig. 2): the "articulare" (Figs. 2 and 6, *ar.*) now embraces the internal angular process. The elements of the posterior sphenoid, and of the auditory capsule, all these are becoming more closely related to each other.

The hyoid cartilages (Fig. 7) are only larger, altogether, than in the ripe embryo; the elements of the mandible (Figs. 2 and 6) are now closely similar to what is seen in the adult Lizard or Crocodile.

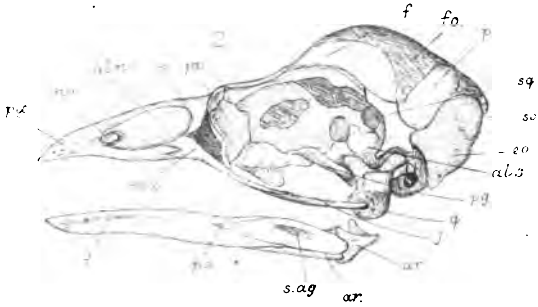
The fifth stage is that of the young Rook (*C. frugilegus*), ready to fly. It is replete with interest.

The basal and lateral elements of the occiput, &c. (Plate XXXVIII., Figs. 1, 4, *e. o.*, *b. o.*) have coalesced; but the upper piece (*s. o.*) still forms an elegant, distinct keystone above (Plate XXXIX., Figs. 1 and 2). The basi-temporals have coalesced with the overlying "para-basisphenoidal" ossification, and at their outer angle the internal carotids (Fig. 1, *b. t.*, *i. c.*) are seen entering.

The fore-part of the "sphenoidal rostrum" (*r. b. s.*) is embraced by the forks of the vomer (*v.*), and its sides by the "meso-pterygoid process" of the pterygoid, and by the "post-palatine processes." The "trans-palatine" (*t. pa.*) is still solid cartilage, without bony deposit; it is a large and well-defined morphological element.

The grooved upper surface of the vomer receives the thick part of the base of the septum-nasi; this base is then two-winged, and these wings are a part of each trabeculæ, unaltered in shape although coalesced together. In front of this part is the shrinking "præ-nasal rostrum" (*p. n.*) which lies between the recurrent ali-nasal folds.

The ali-nasal floor (*al. n.*), over which lies a "turbinal," is seen on each side of the base of the nasal septum; behind, it is continuous with the vomer on each side, the moieties of which are grafted upon the cartilage.



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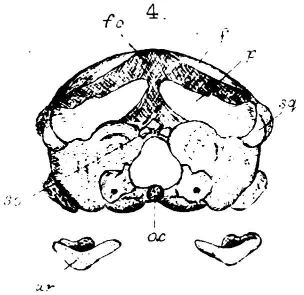

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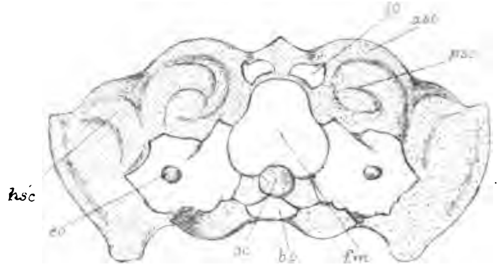

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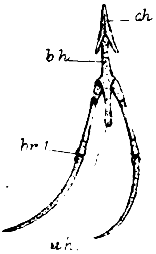

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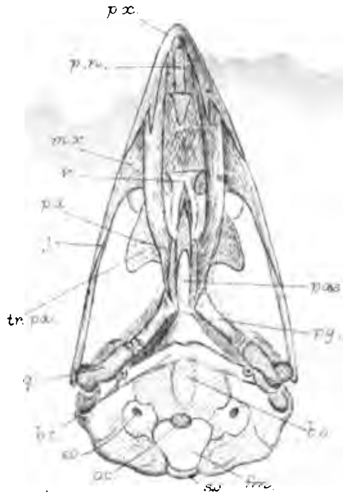

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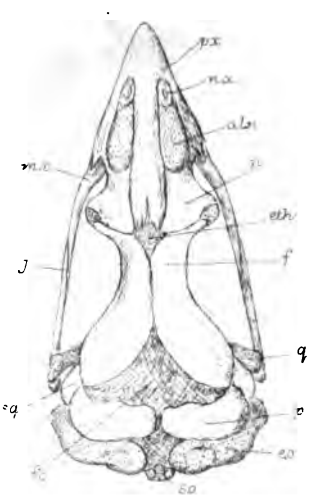

W.K.P. del. ad nat. G. West [illegible]

W. West & Co. imp.

Development of Crow's Skull

The large coalesced præ-maxillaries (*px.*) are seen overlapping the maxillaries (Plate XXXVIII., Fig. 1, *mx.*), and the retral process of these latter bones has now acquired its elegant ear-shape. Seen from above (Plate XXXVIII., Fig. 2), the cranial roof is well filled in; a little of the ethmoid (*eth.*) is seen, and two remnants of the great fontanelle (*fo.*); and the top of the "ecto-ethmoid" (*e. eth.*) projects on each side; afterwards it gets its own osseous centre, distinct from that of the "meso-ethmoid" (Figs. 2 and 4, *pe.*), and from that of the "pars plana" (*p. p.*). This latter part is still unossified (see also Fig. 2). The lachrymal (*l.*) now occupies nearly all its anterior edge. The "meso-ethmoidal" bone (*p. e.*) nearly reaches the notch (Fig. 4, *c. f. c.*) in front, and the interorbital fenestra (*i. o. s.*) behind. The septum-nasi is still soft, as also all the anterior sphenoidal region, which has its orbito-sphenoidal lips very small. In more advanced young I find a distinct præ-sphenoid perched above and between the huge middle ethmoid and basi-sphenoid, but no orbito-sphenoids; the orbital plates of the frontals do duty for them in these high, perching types. The auditory capsule (Fig. 4) has coalesced with the ex-occipital (*e. o.*) and its own opisthotic (*op.*) and prootic (*pro.*) pieces are no longer distinct; the ali-sphenoid (*al. s.*) is now well ossified; the knob of cartilage projecting from it in the post-frontal region is part of the ear-sac—the "sphenotic."

The double nasal sac seen from above (Plate XXXIX., Fig. 3) shows the ali-nasal, ali-septal, and ali-ethmoidal regions (*al. n.*, *al. s.*, *al. e.*); in the latter is seen the broad top of the "meso-ethmoid" (*p. e.*). This part ends as a blunt spur, above the "orbito-presphenoid" (*o. s.*). The transverse sections are through the middle ethmoid, and behind the lateral ethmoids in Fig. 5; and through the unossified part of the middle ethmoid in Fig. 4. This latter section is through the upper and lower turbinals (*u. tb.*, *l. tb.*); the "middle turbinal" is a mere rudiment on the front of the "pars plana," or antorbital. This same section is through the nasals (*n.*), nasal processes of the præ-maxillaries (*p. x.*), the maxillaries and their maxillo-palatine processes (*mx.*), the forks of the vomer (*v.*), and the palatines (*pa.*). A longitudinal section outside the septum (Fig. 6) shows the ali-nasal turbinal (*a. n. t.*), the inferior turbinal (*l. tb.*), and the upper turbinal (*u. tb.*).

The mandible (Figs. 2 and 4) shows the "articulare" much more ossified, the investing elements much more developed; Meckel's cartilage (*mk.*) nearly hidden; and the coronoid (Fig. 4, *cr.*) ankylosed to the "articulare."

The "os hyoides" (Plate XXXIX., Fig. 7) shows an osseous tract in each "cerato-hyal" (*c. h.*), and both the pieces of the third post-oral (*br.* 1) have thin osseous shafts. The azygous piece (*bh.*, *uh.*) is still soft.

After another week or two the pointed fore-end of the pterygoid becomes detached as a distinct "meso-pterygoid," which after a few weeks coalesces with the palatine. The "trans-palatine" cartilage gets endostosis, completely ossifies, and then ankyloses with the palatine, so also do the vomerine forks; and at their fore-end, on each side, a new bone appears—the "septo-maxillary."

The septum-nasi ossifies in some degree, but most of the nasal labyrinth is permanently cartilaginous, yet the ecto- and meso-ethmoidal elements are well ossified and quite ankylosed. Almost every part of the skull and face undergoes this intensive and extensive ossification, but the quadrate bones and pterygoids keep distinct from their surroundings, and the palatines from each other behind. In their "ethmo-palatine" regions they are tied together by the vomer which coalesces by its forks; in front the palatines are fused with the rest of the face. The twisted, fusiform lachrymals are permanently distinct, and a trace or two of sutures can be seen over the "cranio-facial hinge." All the *twelve* pieces of the lower jaw become one V-shaped bone.

In the second post-oral the medio-stapedial is ossified, and all but the tips of the distal cerato-hyals; the upper ends of the third arch, and the greater part of the "uro-hyal" remain soft.

In conclusion, if anyone shall ask me why this multitude of bones should so soon melt into so few? I have only one answer, namely, that the reason lies, I suppose, first, in their genetic history in the "auld lang syne," and then in slow adaptive modifications during epoch after epoch.

DESCRIPTION OF PLATES.

Plate XXXIV.—Development of skull in Hooded Crow (*Corvus cornix*), fifth or sixth day of incubation.

Plate XXXV.—Figs. 1–6. Skull of Rook (*Corvus frugilegus*), eighth or ninth day of incubation. Fig. 7. Skull of Carrion Crow (*Corvus corone*), end of incubation.

Plate XXXVI.—Skull of *Corvus corone*, end of incubation—continued.

[It is necessary to state that only three of Mr. Parker's Plates—Plates XXXIV., XXXV., and XXXVI.—appear in this number of the Journal. The remainder will be given in the succeeding number.—Ed. 'M. M. J.']

(This vol., p. 253

III.—*Remarks on the Resolution of the Nineteenth Band of Nobert's Plate by certain Objectives, especially by a new Tolles's Immersion $\frac{1}{18}$th.* By Dr. J. J. Woodward, U. S. Army.

(*Taken as read before the* Royal Microscopical Society.)

In the 'Monthly Microscopical Journal' for September, 1871 (p. 150), I published a short note on the resolution of *Amphipleura pellucida* by a Tolles's immersion $\frac{1}{6}$th. In a subsequent note dated November 21, 1871, and published by the same Journal in January, 1872 (p. 27), I mentioned that I had engaged Mr. Tolles to construct a higher power objective for the Museum, which had not yet been received, and remarked, "should it fulfil Mr. Tolles's expectations it will give me as much sincere pleasure as it can to him or any of his friends, and I will promptly make the facts public." The objective finally supplied in accordance with this order was put into my hands in August of this year. Mr. Tolles, however, informs me that the optical combination was completed and tested by him in May. This objective is so surpassingly excellent on the plate, that I hasten to make the facts public, as promised.

The objective is made to work wet or dry at pleasure. Wet, it is marked by the maker $\frac{1}{18}$th. The dry front at fifty inches distance from micrometer to screen magnifies 800 diameters uncovered, 900 covered. Angle of aperture 125° uncovered, 155° covered. The wet front can also be used dry for the first half turn of its screw-collar from the extreme open point, the range of powers and angles being very nearly the same as with the dry front. The next half turn of the screw-collar is intended for work wet, the maximum angle as measured in air being above 170°. An arrow on the tube of the objective, and a zero on the screw-collar, indicate the limit proposed for uncovered wet. At fifty inches distance the powers are 900 diameters when the collar is brought to the zero point, 1050 diameters when completely closed. On trial, however, I could not satisfy myself that the zero point was accurately marked. I first noticed that on several slides of diatoms, mounted with extremely thin covers, the best definition wet was obtained when the objective was opened to the zero point, or even yet farther; then testing the objective wet on a broken Nobert's plate, mounted with the lines uppermost and uncovered, I found that the bands were best seen when the correction was one-sixth of a whole turn more open than the zero point marked. At this correction the magnifying power at fifty inches distance was 845 diameters. At ten inches by lamplight it was 170 diameters, the best correction on the naked plate by lamp being sensibly the same as by sunlight, or if anything a trifle more open. I made the latter determinations with the utmost care for the benefit of those who think the several fronts of objectives should be named by their performance uncovered measured by lamplight at ten inches, and who attach an extreme importance to these conditions, which I confess I do not appreciate. Nor do I attach any blame to the distinguished manufacturer by whom this objective was constructed for the trifling deviation from his intended standard; and I may add that having published the above facts, I consider my duty in the matter discharged, and whether others choose on the above data to call the objective $\frac{1}{17}$th or $\frac{1}{18}$th, I shall continue to do as I have done in similar cases, namely, pending the adoption of a rational system of nomenclature, I shall continue to designate it by the name given by its maker, and having published its actual magnifying power under definite conditions, shall confine myself to a description of the work it will do.

With regard to the performance of the dry combinations of this objective, I will merely say that they gave me the striæ of *Amphipleura pellucida* rather better than any dry objective I have ever tried. I have not tested them as yet to any extent on other objects, because of the manifest superiority of the immersion work.

The behaviour of the objective, when used wet, is certainly admirable. In illustration I forward a new series of views of Nobert's plate from the lowest to the highest bands taken by it. These pictures, which certainly excel all my former work on the plate, were purposely taken from the ordinary thick-bottomed plate, accessible to most microscopists, in order that those interested might compare what they can see by their objectives with the work now submitted. Most of the negatives were made with a power of about 1200 diameters; but as paper prints of the 19th band, with this power, are so indistinct as to be practically valueless, I have added one of the 19th band from a negative taken with about 1800 diameters, one of the 15th, for comparison, taken at the same distance, with the same cover correction; and lastly, an enlargement of the 19th band picture, which I hope will serve to illustrate the subject of spurious lines on the edges of the band, as well as to display the fine resolution of the real lines obtained by the objective employed.

It will of course be expected that I should say something of the comparative merits of this new objective of Mr. Tolles, and the immersion front of the Powell and Lealand $\frac{1}{18}$th, which has done so much good work for me since 1869.

Certainly I must give the new Tolles's objective the preference on the plate, and on *Amphipleura pellucida*, both by sun and lamplight. On other tests I have not as yet done enough work to particularize. As to magnifying power, it will be remembered I have already published the fact that the wet front of the Powell and Lealand's $\frac{1}{18}$th, belonging to the Museum, magnifies at its uncovered point 900, at covered 1100 diameters, at fifty inches from micrometer to screen.

Yet these figures would not give the true relations between the magnifying powers of the two objectives as actually used; for that of Mr. Tolles will work through thicker covers than that of Powell and Lealand, and for any given cover works nearer the open point, so that its magnifying power on covered objects is less than might be anticipated from a comparison of the open points of the two objectives.

I have next to state that, Mr. Tolles having been kind enough to send me during the past year a number of objectives ranging from $\frac{1}{10}$th to $\frac{1}{20}$th for study, I have since February of the present year satisfied myself that several of them resolved the 19th band of the plate in a very satisfactory manner. These objectives differed from each other in many particulars, not merely in construction, as to which I know nothing, but in performance. My examination of these objectives has satisfied me that a high angle, with its accompanying advantage of separating power, can be used with less detriment to working distance, penetration, flatness of field, and other advantages usually supposed to pertain to low angle, than has hitherto been supposed, provided the glass is left much under-corrected for colour.

I observed that those of these glasses which were quite under-corrected as to colour, not merely gave the best photographs, but did the best work by lamplight. This result I may say corresponds with what I have been able to observe also with certain objectives of Powell and Lealand, Hartnack, and Gundlach. I do not claim this observation as a new one, for I have reason to believe that the fact has long been well understood in various quarters, but if I am not misinformed the average purchaser of objectives demands approximate achromatism above all else. Now, in view of the irrationality of dispersion, absolute achromatism is impossible, and in aiming to approach it as closely as may be the corrections for spherical aberration are inevitably sacrificed. It appears important that this fact should be more generally known, and I have devised the following simple test, which may serve to illustrate the subject:—

If the microscope be placed horizontally on the window-sill of a dark room, and illuminated by a solar mirror or heliostat, placing a plano-convex lens of about two inches in diameter and ten inches focal length so far behind the achromatic condenser that the rays cross in the principal focus just before they reach the bottom lens of the condenser, it will be found possible with any glass of $\frac{1}{10}$th of an inch focal length or less, combined with suitable eye-piece and distance, to project images on a white cardboard screen with powers of from 1000 to 10,000 diameters.

Now let the test object be *Pleurosigma formosum.* It will be found quite impossible to get a distinct view of the beads with any considerable power, say 3000 to 5000 diameter or upwards, unless the colour corrections of the lens are such that the beads appear of brilliant red on a greenish ground, that is, unless the objective is considerably under-corrected. When this is the case good photographs can be made after the interpolation of an ammonio-sulphate cell, but when the glass is more nearly achromatic not merely the photographs are unsatisfactory, but with white light it will be found impossible to separate the beads distinctly on the screen.

I do not wish to lengthen unnecessarily this already long article; but so much has been said of late with regard to "the advancing powers of microscopic definition" that a few further facts and considerations appear to be demanded.

I note, first, that certain Continental makers would appear to have worked out the problem of high-power definition to about the conclusions indicated above. I have had recently in my hands a new No. 10 immersion by Hartnack, the property of Dr. A. Litton, of St. Louis, Missouri, which handsomely resolved the 19th band, and gave on *Pleurosigma formosum* the colour indications commended above. This objective measured at fifty inches 700 diameters uncovered, 770 covered.

I have, moreover, recently examined a new Gundlach No. 8, the property of Mr. W. H. Walmsley, of Philadelphia, which also resolved the 19th band in a satisfactory manner. This objective measured at twelve inches distance 200 uncovered, 220 covered. Tested on *Pleurosigma formosum* the colour corrections were almost identical with those of the Hartnack objective.

I may add to the above, that during the past six months, four objectives by Powell and Lealand marked $\frac{1}{16}$th have passed through my hands. Three of these were at the time the property of a well-known importer of optical apparatus, the fourth had been made to order for a distinguished microscopist in New York. All these glasses compared favourably with the Museum $\frac{1}{16}$th, though there were some small differences in magnifying power. All had the same colour corrections. I saw, however, in none of them any superiority over the objective furnished me in 1869.

It seems probable to me therefore that the distinguished makers last named have made no substantial progress since 1869, and this view is confirmed by Dr. Royston-Pigott, who mentions in this Journal, February, 1872 (p. 66), that Powell and Lealand had placed at his disposal "for a fortnight" a $\frac{1}{16}$th "similar, though *perhaps slightly* superior, to the celebrated immersion $\frac{1}{16}$th signalized by Dr. Woodward," and which he says the makers were "unwilling to dispose of," on account of its excellence.

It would appear therefore that the recent advancing powers of microscopic definition have, as a rule, since 1869 at least, been rather shown in the advance of various makers to the highest standard then attained, than in any actual progress beyond what had then been done. This qualified advance should, however, be most satisfactory to microscopists, since it promises a competition likely to bring down the present extravagant prices of good objectives.

But beyond this general movement, which is so creditable to all concerned, I must believe that Mr. Tolles* has been so fortunate as to make a real advance, and I shall continue to hold this belief until some one can produce an objective which, in my hands, or those of some other observer, shall succeed in doing better work on the plate than that herewith submitted.

In conclusion, I mention that the price paid Mr. Charles Stodder for the objective above described was one hundred and seventy-five dollars U. S. currency.

IV.—*Aperture of Object-glasses.* By F. H. Wenham.

In declining to continue a discussion directly with Mr. Tolles on the question of angle of immersed objectives, I meant no discourtesy, fully appreciating the temperate manner in which he has endeavoured to maintain his point. My withdrawal was in consequence of his diagrams,† in which rays were drawn through lenses in a direction so adverse to my belief in optical theory, and against plain rudimentary principles that I never could abandon, further argument therefore became useless.

But as I agree with the remark in the last Journal, that the question of aperture has not been "quite satisfactorily determined," I offer some further information. The vexed question has been extended over such a long period, and scattered over so many pages, that the main point at issue from the first announcement of the fabulous increase of aperture of immersion lenses has perhaps faded from memory. I therefore condense the principle in a brief demonstration. Let *a*, *b*, represent the maximum working diameter, or actual surface of the front lens, that is, sufficient for the passage of a pencil of rays of 170° from the focal point *c*. By the law of refraction these rays will take the direction *d*, *e*, in the body of the front lens, completely filling the nearly hemispherical back surface to its edge. Now let the front lens be immersed in water. This of course lessens the refraction at *a*, *b*. The focus is extended to *f*, and the angle diminished to 98°. Still the rays after the first refraction take the same direction, *d*, *e*.

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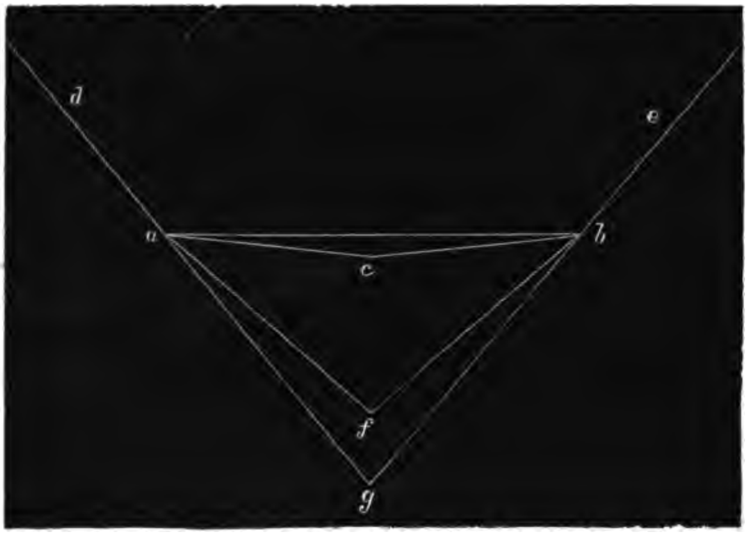
* These words—"that Mr. Tolles"—are omitted from the MS., but we presume they were intended to have been used by the author.

† Page 117, 'M. M. J.,' March, 1872.



Instead of water, let us immerse the lens in Canada balsam. The index of this being taken as the glass of front, *no refraction* will occur at the first surface, but the rays will proceed straight in the direction *d*, *e*, extending the focus to *g*, and reducing the aperture near to 80°. In this demonstration it is needless to complicate the question by carrying the rays through the entire combination, as we may take them at their final emergence at the longest conjugate focus possible, or just within parallelism. *d*, *e*, *c*, must in that case be fixed in direction and position for the dry lens, *d*, *e*, *f*, for the water lens, and *d*, *e*, *g*, for the balsam lens.

If we *decrease* any of the focal distances, *c*, *f*, *g*, from the front surface, so as to increase these angles, the conjugate focus must become extended correspondingly; and having already taken the extreme length, the posterior rays would no longer be parallel, but *divergent*. The effect in this case would be negative, and no image be formed in the eye-piece. Now, with a shorter conjugate focus at the back, perfect images are obtained; but the points *c*, *f*, *g*, would be more distant, and less aperture the certain consequence. We now come to the question of the immersion *versus* the dry principle. This simply means that a film of air shall intervene, either at the surface *a*, *b*, or elsewhere in the cone of rays through the medium. For the sake of simplicity, we will take this film as one of infinitesimal thinness—a case of cover contact in fact. Now, when the refractive index of films of air, that of water, and again of balsam or glass, are dragged in as an attempt at a would-be scientific demonstration, I denounce it as a muddle and downright nonsense. Thin parallel plates of either of these intervening in the cone of rays will in no way alter the angle, as they always emerge in their original direction. A degree of lateral displacement according to the thickness is the only result; and taking, as I have here done, the constantly occurring case of an infinitesimally thin film of air (as when the object-glass is in contact with the cover), neither calculation nor diagram will show a perceptible difference in the angle, which is the *same* whether the air is there or not, wet or dry.

I may put forth this statement without being deemed arrogant by those conversant with optics. The ground is safe, and anyone that ignores or denies such a definite principle must expect to forfeit all respect for his optical knowledge.

Let us now proceed to the indefinite, wherein the best of us are still at fault. I allude to the present unsatisfactory mode of measuring the angle of aperture of object-glasses by means of the well-known "sector." For anything beyond 90° this is by no means trustworthy. The microscope body with the objective is swung round from a pin near the focal point as a centre, and the measure of the angle of the sweep is indicated by the light from a flame some few feet off bisecting the field of view in the eye-piece. With large apertures a definite bisection cannot be obtained, and the limit has to be guessed at by the position at which the light seems to vanish. A border of diffused light frequently exists, and is thus often mistaken for aperture. Under the most favourable circumstances this mode of measuring extreme apertures is so faulty that when statements are brought forth such as 172° "*and a half,*" I cannot help smiling at its absurdity, as a mere piece of brag. This inaccuracy has long been known, and attempts have been made from time to time to provide a remedy. I consider that the most correct of all is that proposed by Professor Robinson, which consists in passing the parallel rays of the sun through the back of the objective, and then by means of a white screen in a dark room or camera intercepting the rays as a disk of light. The angle taken from the diameter of this to the focal point will give the true aperture. Any extraneous or diffused light is either dispersed, or can be detected from its different appearance on the screen. The reason why this plan is not generally used is on account of the trouble attendant upon the arrangement.

Effective aperture of course means the production of a *distinct image* up to the extreme marginal ray. If this image at last becomes so distorted as scarcely to merit the name (such is frequently the case), it may as well be stopped altogether, as the object-glass will perform better with a less angle.

Professor Govin has proposed to associate the measurement of the angle of aperture with the simultaneous view of an object distinctly defined, like the flames of two candles placed asunder, or two white strips separated on a black screen, to the limit of *distinct* visibility; the angle from these two points to the focus of the object-glass will represent the aperture. The microscope is thus converted into a kind of telescope, by means of a pair of lenses over the eye-piece similar to what is known as Ross's "examining glass." Unfortunately for the success of this plan, different optical combinations at the eye-piece give different results by elongating or shortening the conjugate focus, otherwise the measurements would be very satisfactory. I am in hopes of finding a remedy for this. Proposals have also been made for testing aperture by placing a lamp-flame at the back of the object-glass, which remains a fixture, and rotating a compound microscope round the coincident foci as a centre, and measuring the arc traversed. This is exceedingly inaccurate, as the aperture of the rotating glass is superadded to the other. I am, however, constantly in the habit of using this plan for testing the perfection of the image formed by the oblique pencils, in order to ascertain whether they are good enough to remain or should be stopped. When the aperture of the object-glass is very large, and the marginal pencils free from error, a singular optical illusion is produced. On looking towards the front of the object-glass under test, in a line with its surface, a distinct image of the flame will appear to stand out suspended in air; this appearance is caused by the degree of aperture or the oblique pencils of the examining microscope forming an image where none should apparently be seen. I offer these remarks as a caution that whether apertures are tested either in air or water, errors are liable to creep in. At present I have no remedy to suggest, as the investigation remains to be completed.

Since writing the above, I have to announce that Mr. Tolles' object-glass has been placed in the hands of Dr. Lawson. I shall not, however, test the aperture in either water or balsam, till the return of Professor Markoe from the Continent, in order that he may be present, by which time everything will be in readiness, as I perceive that a special contrivance will have to be adapted for the aperture in balsam, to avoid drowning the glass, or prevent it from getting into the fittings, as I should not like to take the risk and trouble of cleaning it out again. This may be avoided by confining the balsam to a small spot on the front, leaving the sides intact.

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of Connective Tissue and Tendon.—A paper contained in M. Brown-Séquard's 'Archives de Physiologie' for 1869, by M. Ranvier, has proved to be the starting-point for a new series of researches on the structure of connective tissue and tendons, which has recently been taken up by M. Boll and Dr. Mitchell Bruce, and which has led to a description of this tissue materially different from that contained in the text-books of some twenty years ago, as well as from the more modern doctrines of Virchow. The latter observer maintained that connective tissue was originally composed of cells, the walls of which became thickened and condensed, forming the intercellular substance, and ultimately underwent fibrillation. The central portion both of the cells and of the processes given off from them he believed remained hollow, and by their intercommunication constituted a canalicular system, ministering to the nutrition of the tissue. The researches of Recklinghausen on the cornea to some extent effected a modification of Virchow's views, by showing that the true cells of that membrane did not form, but were contained within, the stellate cavities and spaces, through which they moved by virtue of their amœboid activity. The researches of Kühne on the intermuscular connective tissue of the frog showed that in this animal the cells of that tissue are destitute of a cell wall, and are not contained in canals, but wander freely through the meshes of the connective tissue. It followed that, in the frog at least, the general idea of a plasmatic system, afforded by the study of the cornea after impregnation with nitrate of silver, could not be extended to the connective tissue generally. Ranvier commenced by examining the tendons in the tail of young rats, which, after removal from the body, were stretched on the slide, kept in position by a drop of sealing-wax, stained with carmine, and acted on with dilute acetic acid. On examination with a magnifying power of 100 diameters, the tissue was found to present parallel continuous red striæ, which, under higher powers, proved to be cylindrical tubes; and these were seen to be crossed by transverse lines, dividing them into nearly equal segments. On slight pressure being exercised, each of these little segments or cylinders split longitudinally, and unrolling itself, proved to be a square cell-plate or lamina, with a nucleus near its centre. Around the cell tubes is an elastic tube or sheath. The tendons are thus, according to Ranvier, permeated by a system of parallel tubes, the walls of which are composed of square cells rolled round till their edges meet, and placed end to end. The stellate figures insisted on so strongly by Virchow as representing corpuscles, he regards as being merely sections of the superficial layer of connective tissue investing the several fasciculi of the tendon, and in these septa his cellular tubes run. The stellate figures brought into view by staining in the subcutaneous connective tissue he also regards as purely illusory; and endeavours to prove it by producing artificial œdema with injection of various fluids into the living animal, and then freezing it before making sections. A somewhat different view has been lately advanced by Boll. Boll describes the cells of connective tissue as granular, elastic, quadrilateral plates, which are arranged in rows and bend around the fasciculi of the tendon. Each plate has a median stripe, which he calls the elastic stripe, which is caused by a thickening of the cell substance parallel to the long axis of the tendon, and is stained deeply with carmine. It is evident that a row of cell plates will give a corresponding row of such stripes—that is, a continuous stripe; and this elastic stripe will assume a straight or wavy appearance, varying with the condition of relaxation of the tendon. Dr. Mitchell Bruce followed Ranvier's method, but gives a different interpretation of the appearances presented. Accepting the views of Boll on the relation of the cells to the fibrillar bundles, he describes fine lines crossing the fasciculi and dividing them into quadrilateral spaces, in the centre of which a nucleus comes into view on the addition of acetic acid; whilst, after a more prolonged influence of this reagent, the fasciculi become considerably swollen and transparent, and the quadrilateral plates present irregular ragged borders. With nitrate of silver staining, an investment of cells, beneath which a system of serous canals appeared in some places, is brought into view. The cells are rendered still more distinct by the chloride of gold method, or by a combination of the silver and gold methods. His view is that the cell elements in the young tendon are plates so arranged as to bend round the fibrillar bundles, and that they consist of granular protoplasm enclosing a nucleus. The cell plates are of nearly uniform size, and by their union form a connected whole, which may be aptly likened to a cloth composed of a number of stripes; and the fibril bundles are so enveloped in this cloth that each is surrounded to the extent of half its circumference by one stripe. As we understand it, the continuous expansion pervading the tissue formed by the cells resembles a sheet of corrugated iron. If two such sheets were superimposed on one another longitudinal spaces would remain, and these in the tendon are occupied by the tendinous fasciculi.—*The Lancet*, September 7.

The Degenerative Process in Nerves.—The last part of Virchow's 'Archiv' contains a paper by Dr. Benecke, of Königsberg, under the title of "Histological Processes in Divided Nerves." His investigations, he says, have extended over many years; and he has experimented upon cats, rabbits, crows, fowls, pigeons, and various small birds. Of all these he finds young cats the best adapted for research, as the nerve trunks in those animals contain very little connective tissue in their interior. The mode of operating consisted either in dividing the nerve with the knife, or in applying a fine silk ligature for a few moments, which is almost equally efficacious in cutting through the axis cylinders. The changes induced were examined at various intervals of time, ranging from two hours to six months, and both in the fresh state and after maceration in different preserving fluids. The first changes observed, and these take place within a few hours after section, are that the two extremities of the nerve adjoining the cut become of a greyish or yellowish-red colour, cloudy aspect, and more or less strongly swollen. These changes are due, says the 'Lancet,' Sept. 21, 1872, to congestion of the severed blood-vessels, and to the imbibition of the serous transudate. In a short time the part of the centric extremity lying near the cut and the whole peripheric part assumes a cloudy-yellow tint, becomes somewhat attenuated, and tears easily. The sheath of Schwann undergoes, at first at least, very little alteration; the medullary sheath, on the other hand, coagulates, and breaks down into a finely granular detritus. The axis cylinder early appears enlarged and swollen, but soon attenuates, and ultimately disappears entirely. In the later stages of the process of disintegration it is impossible by any means to bring it into view. Benecke has never been able to find that the degenerative process progresses in an orderly manner in either direction from the lesion; but believes that it takes place *coincidently* throughout the whole length of the peripheral extremity, and in the immediately adjoining portion of the centric end. But even whilst disintegration is advancing, the process of regeneration has commenced, in the form of multiplication by fission of the nuclei of the neurilemma, or sheath of Schwann. These soon become converted into elongated fusiform bodies, and after a time form the only contents of the empty and collapsed sheaths, which then, he thinks, disappear. The protoplasmic processes of the new elongated nuclei now undergo fusion, and unite to form pale slender bands coincidently in the cicatrix and in the peripheral end of the divided nerve, which is thus brought into organic connection with the central stump. In the further progress of the regenerative process a medullary sheath is gradually formed around the primitive bands or cylinder axes originating from the nuclei. These last no longer retain their ordinary appearance, but become homogeneous, a few only remaining in each fibre, from which the normal sheaths of Schwann are afterwards developed. Upon the whole, therefore, it would appear that after section the proper nervous tissue of the entire peripherical portion of the nerve undergoes disintegration and disappears, whilst regeneration is effected by a process precisely analogous to that which many observers have demonstrated to occur in the natural development of the nerves in the tail of the tadpole and in the embryo of the fowl; the cylinder axis being first formed by the coalescence of nuclei, to which the white or medullary substance and the sheath of Schwann are subsequent additions.

Muscle as a Constituent of Nerve Tissue.—Signor Tigri has shown the presence of muscular fibres among the nerve fibres of the cerebrospinal nerves and the ganglia of the sympathetic.* When an irritation is applied, contraction is caused, and the liquid contents of the nerve fibres are caused to vibrate, and the motion is conveyed thus centripetally or centrifugally. In reference to these anatomical and physiological peculiarities, he calls the ganglia of the sympatheticus magnus, as well as the large ganglia of the brain, "nerve hearts." He considers that the so-called Remak's nerve fibres are muscular fibres.

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* 'Gaz. Med. Ital.'

Grey Substance of the Cortex of the Cerebrum.—The structure of this substance has been quite lately investigated by Prof. Gerlach.* He applied the gold method as a hardening means, and found—1. That besides the well-known white medullated nerve fibres running from the white into the grey substance, and which are arranged in fasciculi radiating towards the periphery, there are also numerous horizontally-running, medullated fibres, which communicate both with one another and with the radially-disposed fibres, forming a coarse plexus visible even with a power of sixty diameters. 2. That in the meshes of this plexus are contained the ganglion cells, and a second far finer plexus of extremely delicate non-medullated fibres which can only be brought into view with the highest powers of the immersion system. This second plexus is formed of the finest protoplasmatic processes of the nerve cells, whilst the medullated fibres appear in part to take their origin in it, the individual fibres becoming surrounded by medulla. 3. Some of the protoplasmatic processes of the ganglion cells develop directly into medullated fibres without branching, and thus such fibres originate partly from the cells themselves, and partly from a plexus of non-medullated fibres.

Brain Tissue in General Paralysis.—The 'British Medical Journal' states that at the meeting of the British Medical Association, Dr. J. Lockhart Clarke exhibited microscopical sections of brain tissue illustrating general paralysis; and he explained that he had found the conditions of brain tissue to be in the first place that of wasting, then a widening of the perivascular canals, and a separation of the sheath of the same. This sheath (apparently hyaloid) becomes corrugated, and contracts round the vessel, which is seen to run within it in a tortuous course, the tortuosity often being so great that the vessel becomes twisted into knots or kinks. Dr. Clarke further explained how he found the white tissue disintegrated, and the grey matter wasted; the grey cells lost their sharp contour; and their granular contents, as a little heap, ready to fall asunder.

Whence Come Fat-cells?—This interesting subject has been lately taken up abroad, and a very interesting summary of the result is given in the 'Lancet' of August 17. The question of the origin of the fat present in every well-nourished person is not easily answered. Putting aside the debated point of the direct conversion of the carbohydrates into fat, there are three theories that have obtained a certain amount of credit. The first—resting on the fact that, with free supply of oleaginous food, an abundant deposit of fat takes place in the subcutaneous connective tissue, in the omentum around the kidneys, and elsewhere—maintains that the fat enters the economy in the ordinary manner, and is directly stored up in the interior of the fat-cells. This view entirely ignores the mechanical and physiological difficulty of the passage of an insoluble substance through the cell walls. The second view rests on the now well-ascertained fact that albumen and its analogues are capable of undergoing disintegration, in the course of which, besides other substances, fat is certainly formed. Nothing can demonstrate this more clearly than the growth of the eggs of the common bluebottle on meat or in defibrinated blood. It is therefore maintained by those who hold this view, as Toldt and Subbotin, that the cells originally take up soluble albuminous compounds, which, undergoing disintegration within the cells, split into certain compounds that readily escape from them, and fat, which remains behind. These authors hold that all the fat of the food is consumed or burnt off during its passage through the economy, and is only exceptionally applied to increase the natural stores present in the body. A third view, to a certain extent intermediate to the other two, is held by Radziejewski, who maintains that an important proportion of the fat of the food is converted into soap by the pancreatic juice and the alkali of the biliary acids, and in this state easily traverses the intestinal epithelium, as well as the membranes of the proper fat-cells. These cells, again, have the power of converting the soaps into free fat, which remains in their interior. This view is certainly supported by various micro-chemical and physical researches showing the importance of saponification in facilitating the passage of fats through animal membranes. Radziejewski himself has demonstrated—first, that soda soaps are absorbed in their passage through the intestines; secondly, that soaps made of oils foreign to the economy are deposited but slightly, if at all, in the form in which they are ingested. Whilst the experiments of Matteucci and others, showing the effects of alkalies on the osmosis of fats, are well known. The whole subject is well reviewed by M. Hofmann in a paper just published in the 'Zeitschrift für Biologie,' giving an account of a series of experiments in which he attempted to ascertain whether any, and if any, how much, fat was stored up from the fat consumed in the food. With this end in view, he starved a dog till it had consumed all, or nearly all, the fat in its body. It was then supplied freely with fat mingled with a small quantity of albumen. After a few days it was killed, and the total amount of fat in the body determined. A certain amount, it is evident, might proceed from the decomposition of the albumen; but if the quantity found exceeded this amount, estimated as highly as possible, it is equally clear that the excess must proceed from the fat consumed as food. This, however, did not prove that the fat was stored up in fat-cells, since it might be contained in the blood; and a special set of researches were instituted to show that the blood, under these circumstances, was not extraordinarily rich in fatty compounds. The general result of M. Hofmann's experiments, as far as they have yet gone, seems to prove definitely that the fat stored up in the body cannot possibly be entirely derived from the albumen consumed as food, but must be in part obtained from the fats. M. Hofmann does not, however, attempt to explain the mode in which the fat gains entrance into the cells of adipose tissue—a point on which we are at present completely in the dark.

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* 'Centralblatt,' No. 18.

NOTES AND MEMORANDA.

Misnaming Objectives.—The following note from Mr. C. Stodder appears with the above heading in the 'American Naturalist' for August: we have added to it some remarks from Mr. Wenham:—

"The brief remarks of mine, printed over the initials C. S. in the March number of this journal, were copied essentially in the 'Monthly Microscopical Journal' for April. In the May number of that periodical Mr. Wenham writes a reply. It is a remarkable paper, not only from the eminence of the writer as an authority on microscopy, but from his evident loss of temper, and by the terms to which he refers to Mr. Bicknell and to C. S. Under these circumstances I must ask for a little space for a rejoinder to my share; I have nothing to say for Mr. Bicknell, as he is able to take care of himself. Mr. Wenham commences his paper, which *he calls* a 'reply,' with this,—'to correct a misstatement that I [Mr. W.] wrote a paper in reply to one of Mr. Bicknell's; I did not commit myself to such an extent.' This is a mere quibble, unworthy of its author. The very caption of the paper had Mr. Bicknell's name in it. I should not have noticed this, had not Mr. W. unfairly, as I think, charged me with a misstatement. Next, Mr. Wenham couples C. S. and Mr. Bicknell together as if they acted in concert, and were joint writers. I can assure Mr. Wenham that it is not so. Mr. B. is not responsible for anything I have written, nor am I for him. Neither had seen the other's writing until it was public. Next, I have no 'plea or atonement' to make 'for expressions hastily or inconsiderately written.' My expressions were used deliberately and after full consideration of their import. I still hold the same opinion, namely, that selling an objective by a name that does not *approximately* indicate its focus (*i. e.* $\frac{1}{5}$ for $\frac{1}{4}$, $\frac{1}{20}$ for $\frac{1}{16}$, or, as I have known, $\frac{1}{20}$ for $\frac{1}{40}$, as in the case of an eminent French maker; or, as in another instance, a $\frac{1}{18}$ for a $\frac{1}{30}$; or, as in the case of an English objective that I have recently heard of, a $\frac{1}{20}$ for a $\frac{1}{8}$) is an 'imposition,' or a fraud if that term is preferred, not applying it, however, as Mr. Wenham represents, to a particular firm, but to all, of any country, who practise such 'imposition'; and that Mr. Wenham in his paper, by stating that '$\frac{1}{8}$ths were $\frac{1}{9}$ths or $\frac{1}{10}$ths, and some now approach $\frac{1}{12}$ths in power,' without disapproval, was practically defending the custom, and that he does not now deny. His paper in reply to Mr. Bicknell was published in December. In May he writes, 'no one knows better than myself the difficulty of adopting a nomenclature that shall exactly denote the power of all the highest object-glasses sent out'—something has evidently produced some effect on him since that time. The complaint was not of want of 'exactness,' but of gross misnamers of 20 or 50 per cent., such as he named in the December paper, not in regard to the highest powers alone, but applicable to the lowest powers as well. Such was what I called an imposition, and I call it so now. In the 'Quarterly Journal of Microscopical Science,' October, 1862, Captain Mitchell gives the measurement of the focus of several London objectives, most of them being undernamed. Captain M. complains of this. He says, 'when I buy a $\frac{1}{4}$th, I want a $\frac{1}{4}$th, not something else.'

He calls those correctly named, *honest;* by implication, those not so named, dishonest. Dr. Wm. B. Carpenter* says, 'the designations given by the opticians to their objectives are often far from representing their focal length as estimated by that of single lenses of equivalent magnifying power, a *temptation* to *underrate* them being afforded by the consideration that if an objective of a certain focus will show a test object as well as another of higher focus, the former is to be preferred. Thus it happens that what are sold as ½-inch objectives are often more nearly $\frac{4}{10}$, and that what are sold as ¼ are not unfrequently more nearly ⅕.' I presume that I am justified in assuming that Mr. Wenham was fully aware of both the above, that Captain Mitchell termed the custom dishonest in 1862, and Dr. Carpenter that it was the result of 'temptation' in 1868, yet he did not feel called on to 'practically defend' the want of honesty, or the yielding to temptation. Was he not then as now 'a witness in behalf' of those *he* calls the 'most respectable portion of the body'? Was it only censure from this side of the Atlantic that was 'worth caring for'? It certainly looks so. For some twenty years I have watched Mr. Wenham's contributions to microscopy. I have used and admired his ingenious inventions and appliances, and have looked upon him as one of the foremost leaders and authorities in the mechanical and theoretic departments of the science. It was with regret that I saw that he did not disapprove of the fictitious nomenclature. It is with greater regret that I find that he has in his haste used arrogant expressions. The question of nomenclature is now being agitated, the attention of microscopists is attracted to it, and one consequence will be that the 'honest' makers will be appreciated."

Appendage to Mr. Stodder's Remarks from 'American Naturalist,' by Mr. Wenham.—Mr. Wenham sends us the following reply:—I should not have taken time to notice the above long comment on my short letter, appearing on page 234 of this Journal for May, 1872, but for the remark that my letter was written with "evident loss of temper"! Quite the reverse; it was penned in a spirit of "chaff," and Mr. Bicknell, in his brief note in reply, seems to have caught the vein; at which no one, perhaps, laughed more heartily than myself. On the other hand, it has drawn C. S. out of his shell, with horns erect, in his proper name or colour. I have nothing further to say on the question, which leads to no scientific discovery, and is one to be settled between the makers of object-glasses and purchasers, who are now sufficiently warned. No particular reform can be anticipated by pages of a controversy having from its very basis such full scope for personalities, of which *this* and the above may be taken as a sample. The tone is becoming silly and tiresome; and having contributed my share, I must drop the subject, with the remark that no one would be more willing to induce the makers to adopt a nomenclature having a definite reference to actual magnifying power than myself, could I see the possibility of doing so. Numerals such as those adopted by the Continental makers would perhaps partly meet the difficulty; but I believe that no English optician would consent to name his glasses this way.—F. H. Wenham.

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* 'The Microscope,' 4th edition, 1868, p. 184.

A New Slide for Exhibiting Infusoria is described by Dr. Hunt, who read a paper at a late meeting of the Biological and Microscopical Section of the American Academy of Natural Sciences. In this paper Dr. Hunt called attention to an ingenious slide invented by Mr. D. S. Holman for exhibiting infusoria, especially under the gas microscope. These slides are made by grinding a deep depression in a thick slide, and then making a very shallow circle all round the deeper central cavity. If the central depression be filled with the organization in water which is desired to be studied, and covered with a thin glass, in a short time the shallow circle all round the margin will be filled with minute bodies, often zoospores or other objects in the life history of the object in the deeper cell. The pressure of the atmosphere retains the cover in position.

Photo-mechanical Printing.—Incidentally to a pathological report to the Surgeon-General, says the 'American Naturalist,' Sept. 1872, Dr. J. J. Woodward calls attention to the familiar disadvantages of the usual means of representing in publications the magnified appearance of microscopic objects by etchings, lithographs, and woodcuts. All such hand work is laborious and wasteful of time if done by the investigator, and liable to omit the most important points if entrusted to another artist. Even the microscopist himself, being unable to represent all that he sees, is obliged to select what he conceives to be of importance, and thus represents his own theories rather than severe facts. [If, however, his theories are correct, and his delineation skilful, this very power of selection and construction enables him to give a distinctness and completeness which is lacked by the photographic camera.] The advantage of truthfulness is on the side of photomicrography, but silver prints are expensive, inconvenient, and not permanent, and the reproduction of the negatives in permanent inks is greatly desired. Two such methods are now successfully used in the United States. By the Woodbury method a relief surface of gelatine is produced from the negative by the action of light, and from this a metal "intaglio" is produced by pressure. In this a series of suitably coloured gelatine films, which constitute the prints, are formed by mechanical means. Thus was produced the photograph of *Amphipleura pellucida* in the last April number of the 'American Naturalist.' In the Albertype process a printing surface (not a relief) is produced through the negative on a gelatine film by the action of light. The prints thus produced are, at present, less expensive than the Woodbury prints, and more convenient for book work, but the edition is less uniform. On enamelled paper the prints are handsomer, but will not bear much handling.

The Angular Aperture of Immersion Glasses. — In the 'American Naturalist' for September, Dr. Josiah Curtis says that all lovers of progress in scientific research feel much pleased with the labours of investigators who make good use of the extensive means at their control, as well as also with the results which have emanated from the patient thought and close study of such men as Mr. Wenham and others abroad. "We are very apt, however, to give too little credit to the intelligent instrument-maker himself. The mind which combines science with practice in its application has great advantages, and should be both respected and encouraged. How we have been forced to modify our opinions, since an angle of aperture of, say, 150° in microscopical objectives was considered absolutely unattainable! No doubt the very men who honestly and firmly believed those things impossible which are now quite familiar, were as glad as any one, when they became convinced, by facts, that they had been in error. Equally gratified, probably, will be Mr. Wenham, when he shall see for himself that an angle of more than 82° can be attained through balsam. Within a few days, I have had a good opportunity to see a $\frac{1}{10}$th objective of Mr. Tolles' make give an angle of 92° through balsam with tank arrangement of Mr. Wenham. I feel disposed, however, to let Mr. Tolles speak of this in his own words, the more so as my time is extremely limited:—

DR. JOSIAH CURTIS, BOSTON, *July* 8, 1872.

Dear Sir,—At my request, you were present recently when I measured the angular aperture of an immersion $\frac{1}{10}$-inch objective when immersed in balsam. You verified the results gained at that time. We used the tank method of Mr. Wenham.*

The $\frac{1}{10}$-inch tested, I stated to you, had in air angular ap. of 170° (upwards).

In water we found the angle to be 110° +.

In balsam the angle was fairly 95°, using petroleum lamp flame, thin, for light, in a darkened room. As you will remember I remarked that with sunlight I got two degrees more.

It will, I know, be of interest to you, and I am sure to some others, to hear of results of test of angle in some other cases. I will, therefore, set down here the angle of aperture found to pertain to some of my immersion objectives in balsam, in water, and in air.

	Air.	Air.	Water.	Balsam.
Single front	$\frac{1}{18}$ in.	170°	120°	87°
"	$\frac{1}{18}$ "	170°	110°	88°
Compound front	$\frac{1}{10}$ "	175°	117°	95°
" "	$\frac{1}{18}$ "	175°	105°	
" "	$\frac{1}{6}$ (high)	172°	106°	88°
Single "	$\frac{1}{5}$ in.	175°	127°	110°

The varying differences between the water and balsam angles can, in a general way, be accounted for from the formulas of construction differing considerably, each one from any other.

Of all these objectives the most effective (especially when its low power is considered) is the $\frac{1}{5}$-inch of 110° *in balsam*. This is true of its use for objects mounted in balsam, as Rhomboides, *small*. But notably so as to its work on dry A. pellucida. My London specimen of this, received through the U. S. A. Medical Museum, is resolved into lines that *shine*, I may say. The illumination I used was petroleum lamp flame, no condensation. With the same means *all* the objectives show A. pellucida with the same illumination, but with a difference.

With sunlight and a blue cell no doubt the higher powers would have their proper advantage.

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* See 'M. M. Journal,' August, 1871.

It is proper to mention that the $\frac{1}{8}$-inch of 110° balsam angle was constructed on the plan proposed by me in the London 'Monthly Microscopical Journal' for March, 1872, where I have made use of a diagram by Mr. Wenham of a $\frac{1}{8}$-inch of his construction, to *indicate* modifications such as would give more than 81° or 82° in balsam.

With proper appliances below the balsam-slide (as pointed out by me in the London 'Monthly Microscopical Journal,' for July, 1871), we can with this objective ($\frac{1}{8}$-inch) utilize 110°, instead of about 81°, the limit of the amount of angle otherwise available. *In the first place* the large angle must have access to and through the balsamed object *from below;* in the next place the objective must be capable of receiving and transmitting that dimension of pencil to the eye, which thing previous to my own demonstrations has not been shown to have been done.

With much respect, yours truly,

Robert B. Tolles."

CORRESPONDENCE.

Mr. Stodder and Mr. Tolles, and their Observations.

To the Editor of the 'Monthly Microscopical Journal.'

October 7, 1872.

Sir,—In the July number of this magazine a letter appeared from Mr. Stodder containing strictures on an article I had contributed; of which I took no notice at that time, partly from the intimation it contained that something more would be forthcoming on the same subject. Perhaps, therefore, you would now allow me space for a few observations in reply.

Mr. S. thinks it a breach of literary etiquette that I should have so unceremoniously put his error down to the account of Mr. Tolles instead of himself. That he was in fact prompted by Mr. Tolles, he admits, the prompting being, however, in this case not oral but by printed catalogue. But though, as it happened, correct as to the fact, I had no "right," he considers, to assume it, assuming, as it were, that he has not sufficient knowledge to originate a mistake for himself. But here Mr. Stodder has forgotten—owing no doubt to the multiplicity of his literary works—that he had himself published the information which took him so much by surprise. In the sixth volume of this Journal, at p. 201, he will find notice given, on his own authority, that he is to be considered only as the exponent of Mr. Tolles, and, with special reference to this particular question, that he does not presume not only to express, but even to possess, an opinion of his own.

As Mr. Stodder has not time to read his own compositions, it would perhaps be too much to expect him to read those of others. I had expressed an opinion that a $\frac{1}{8}$th of Mr. Tolles might have an angle as high as 160°. Afterwards seeing Mr. Stodder's announcement that this glass has, with water, an angle of 140°, I intimated, by notes of admiration, that he ought to have known this was absurd and impossible. Bringing these two together, Mr. S. announces his discovery of a fatal contradiction.—"In November he would not be surprised by an aperture of 160°; in April he is astonished by an aperture of 140°." Upon this he proceeds to comment with severe irony that I must have made progress in the study of Optics, and then dismisses my contribution as a "ridiculous lucubration."

Now the thing—the one thing—asserted in this controversy from the beginning is this, that an aperture, whatever it may be when the object is in air, is abruptly changed when the object is immersed: so that the angle of a glass, *the same glass*, is totally different when used in one way from its angle when used in another way. Entirely unconscious that this is itself the "thesis," Mr. Stodder writes to announce his discovery,—that he has detected a contradiction. He is ready at a moment's notice to decide the controversy;—that he does not know, and has never asked, what it is about, does not seem to have given him the smallest concern.

This is Mr. Stodder's contribution to the discussion. It is his own contribution, written without prompting; as from another passage we learn incidentally that Mr. Tolles was absent, and had no share in it.

I have thought it worth while calling attention to this episode, because it is instructive beyond the immediate question concerned. Mr. S. has the pen of a very ready writer. His name is familiar to the readers of many journals in many lands. From his many appearances he has come to be looked on by casual readers, latterly also by himself, as an authority on the more recondite questions of optical science; and has performed the difficult feat of building up a reputation for profound learning on a knowledge derived from the perusal of printed catalogues. His creed is, that on every disputed point Mr. Tolles is right, and the other side wrong: a fidelity the more to be admired when we find him ready to prove his position not only without being able to understand the question, but without even knowing what it is.

As I observe from your editorial notice that Mr. Wenham declines any further controversy with Mr. Tolles, I add a few observations on his lately-published table, the last probably I shall have occasion to make in connection with it. This table Mr. Tolles offers as "proof enough" of the position he has maintained. Now assuming for a moment the accuracy of his figures, of what are they a proof? The position which he undertook, which he attempted by diagrams to prove, and which now in this his latest paper he reasserts in express words, is this, that by an immersion lens *the whole angular pencil can be transmitted.* In proof of this we are offered measurements of nine glasses; and how much do they give? They differ a little, but taking them on the average we find just 90° for each. And this is offered as "proof enough" that the whole pencil, or 170°, can be transmitted.

90° however is something greater than 82°, the limit scientifically assigned. And what this extra 8° shows is only this, that Mr. Tolles cannot be trusted to take an observation correctly. Of this we shall know more when the glass which he has so kindly sent over shall have arrived. Meantime, as a preparation for what we may expect, a comparison of his published figures supplies sufficient material for estimating his accuracy as an observer.

In the first place it will be remembered that, having expressly tested a glass of his own, he wrote to say he had found its angle to be 100°. But on the following day his secretary writes to say that having looked again he found it was a mistake; the angle was really 110°.

Again looking at last table we find him claiming for one of his glasses an aperture of 178°. (By-and-bye, for it is in the nature of Art to progress, we may expect to hear one announced having 179°, to be followed, doubtless, in due time, by another of 180°.)

Again, in the same list (p. 106) we find the figures contradicting themselves. We have, *e.g.*, two glasses with apertures of 175°, having different angles in water, the first giving 117°, against the smaller angle of 113° for the second; but going on to the column for balsam, the reduction is inverted, the second, which was lower in water, being now the higher in balsam,—95° against 93°. *Both* results cannot be true. For the loss for different media is connected by a fixed relation (though not of simple proportion); a diminution for one answering necessarily to a corresponding diminution for the other. All four measurements therefore cannot be right. (As a matter of fact, all four are wrong. The true aperture for both sets is in water 97½°, in balsam a few minutes over 80°; on the supposition, that is, that the angle in air has been correctly taken.)

Further, I have only to observe, that Mr. Tolles in this paper repeats mistakes formerly pointed out, and repeats them without apology or explanation, as if they had never been corrected. Under these circumstances it may perhaps be allowed that as much attention has now been given to these "demonstrations" as can in courtesy be required, and that further notice of them will not be necessary.

It is perhaps not fortunate for the "general reader" that so much of this subject has been elucidated controversially; for the style naturally used in proving a disputed truth is not generally the best that might be chosen for making clear what it is. I think it not unlikely, therefore, that one result of these discussions may be the appearance of some obscurity as to the application of the principle, on account of the variety of combinations which occur. The object, *e.g.*, may have a cover, or it may be without one; it may be in air, or it may be immersed in water, or immersed in some denser medium; and the object-glass may be dry or it may be immersed. These combined in different ways give an appearance of complexity, which however is apparent only, and not real. I therefore take the opportunity of this letter to re-state the principle in its application, which may be done simply enough; for whatever the number of cases, the same invariable principle governs them all.

Every case that can arise comes under one or other of two classes; the object itself is either dry or immersed.

Suppose, firstly, that it is dry. It may be either with or without a cover; this will not affect the aperture. The glass itself may be either immersed or dry, but neither will this affect it; it is only a case of a thicker or thinner front. In all these cases the aperture will be given, with approximate accuracy, by the ordinary method.

Next, suppose the object immersed. It may be covered or uncovered, the glass may be immersed or dry, but in every case alike the common method will fail, and must be set aside. There is a limit, easily determined, beyond which the aperture cannot pass; and this limit will depend on the medium in which the object is placed. Take the angle whose sine is the reciprocal of the refractive index; twice this angle is the limit of the aperture. If we wish also to determine experimentally the actual aperture in a given case, or how much it falls short of the limiting value, we must use the method described by Mr. Wenham, which inexperienced workers will do better not to attempt. Otherwise, however, it may be ascertained also by calculation, by a method which it is not necessary to specify here.

Briefly, therefore, the limitation or non-limitation of the aperture is governed by the immersion or non-immersion, not of the object-glass, but of the object.

Your obedient servant,
S. L. Brakey.

Using the $\frac{1}{8}$th Binocularly.

To the Editor of the 'Monthly Microscopical Journal.'

35, Upper Park Street, Clifton, Bristol, *Oct.* 19, 1872.

Sir,—I was greatly surprised to hear at a meeting of our Microscopical Society that Mr. Wenham was engaged in perfecting an arrangement of his Binocular Microscope, by means of which the $\frac{1}{8}$th object-glass could be employed binocularly, since I have been in the habit of using my Ross's $\frac{1}{8}$th with Mr. Wenham's instrument (the work of Mr. Husbands, of this city) for some time past, with excellent effect. My "modus operandi" consists in withdrawing the prism until the dark outline of its edge crosses the centre of the field exactly, and placing the instrument in such a position that the light falls upon the mirror from the left; by this method, and especially by the aid of the condensing lens and petroline light, I can examine with very satisfactory results objects suitable for the $\frac{1}{8}$th. That the object is seen with both eyes may be easily proved by placing a circular diatom, an arachnoidiscus for example, in the centre of the field, so that the dark line before referred to crosses its centre, and by closing one eye, and looking down the tube opposed to the other, and *vice versâ*, it will be at once apparent that opposite halves of the diatom are seen, and by now employing both eyes simultaneously the diatom presents itself in its entirety, and its markings can be made out perfectly.

Apologizing for trespassing upon your valuable space,

Believe me, Sir, yours truly,
Samuel Smith,
Surgeon.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *October* 2, 1872.

W. Kitchen Parker, Esq., F.R.S., in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors. The Secretary called the attention of the meeting to a series of photographs of Nobert's lines, which had been sent to the Society by Dr. Woodward, of the U. S. Army; the 19th band having been defined by Dr. Woodward with Tolles' lens in a far better manner than in any previous attempts to resolve it. A note from the Dr., not in the room at that moment, would be printed in the Journal, but he (the Secretary) would just state that the lens with which such successful results were obtained is one that is not perfectly corrected for chromatic errors. Lenses more perfectly corrected did not show the lines so well. This was an important fact, and bore out a practice (of many years' standing) by Messrs. Powell and Lealand. Their recent lenses, which are admirably corrected for chromatic errors, have been found better adapted for most microscopical work of a very delicate description, and yet a lens having a certain quantity of chromatic error left in it produced a better photograph of the 19th band.

Mr. Ingpen inquired whether the glass was over-corrected or under-corrected.

Mr. Brooke thought it was probable that the lens was corrected for the rays which have the greatest luminosity, and that the darker ones were left to take care of themselves.

Mr. Ingpen: If there was a slight over-correction for the blue, great power would be obtained in photographic work.

The Secretary said he had received from Mr. Tegetmeier a request that the opinion of the Fellows should be obtained upon a pigeon's feather which presented the abnormal condition of a second growth, or outgrowth upon an already formed feather.

Dr. Braithwaite thought the condition of the feather resembled somewhat that which is seen in the vegetable world, where a continuation of the axis of a leaf takes place, the growing point continuing itself and producing a repetition of the previous growth.

The President concurred with Dr. Braithwaite in his opinion.

Mr. Lowne thought the peculiarity was very much like what was seen in the peacock's feather, where a similar separation of parts was exhibited. The condition was due to the bird being in a state of starvation at the time when the growth was going on. The quill of a feather enclosed a quantity of cellular matter which developed as the quill grew; and in sickness, or when the bird was in a starving state, the cells died. But it was not unusual to see a fresh quill, as it were, growing out of the first quill. The appearance presented was the result of a morbid process in some degree.

Dr. Hudson then read a paper, "Further Researches on *Pedalion mira;* a new Six-limbed Rotifer."

Mr. Slack said that having had the advantage of seeing specimens of Pedalion which Dr. Hudson had kindly sent him, he was in a position to state that the results laid before the Fellows that evening gave no notion of the immense amount of trouble Dr. Hudson must have taken to understand the creatures. Those exhibited before the Society that evening were sufficiently hungry and empty to be comparatively intelligible. The interior organs of specimens he (Mr. Slack) had examined were practically invisible, owing to the presence of a quantity of coloured matter, and they did not live long enough to permit of their being reduced to the requisite condition of attenuation. The creature, in a free-swimming state, exhibits a peculiar jerking motion like *Triarthra*, which was the nearest approximation to it in being furnished with articulated spines. Pedalion's rapid mode of swimming renders it extremely difficult to follow its movements. After alluding to the unsatisfactory definition of the term Rotifer, Mr. Slack continued: If all the oral cilia were taken off from Pedalion, anyone looking at it in that condition would take it for a small crustacean. Many naturalists, Gosse among them, had pointed out the affinities of Rotifers with Articulata, and certainly the creature under discussion approached that type more nearly than any that had been previously found. From the perfection of its swimming organs and their complexity, there was an evident resemblance to the *Arthropodous* type of animals. The specimens he saw had no active cilia at the tail bifurcations, and it was very probable that Dr. Hudson's explanation that they had been shed was correct.

Mr. Lowne said he thought that some forms of Arthropodous Rotifers had been described in Kölliker's 'Zeitschrift' about twelve months ago. These rotifers had what he (Mr. Lowne) took to be Arthropodous legs, and the interior arrangement of the animal had been set forth in nearly the same words as those Dr. Hudson had used in describing Pedalion. The memoir was written by a Russian.

Mr. Stewart said he thought it possible there might be some difficulty in distinguishing between the Rotifers and some of the more minute forms of *Crustacea.* He would only mention one of the definitions to be found in text-books, namely, that no *crustacean* has, in any parts of its body, cilia; whereas these creatures which Dr. Hudson had described in his admirable paper had articulated limbs, and the great majority of them had cilia, although some exceptional forms might be deprived of them.

Dr. Hudson said he had seen the drawing referred to by Mr. Lowne; it had been published shortly after he (Dr. Hudson) had made his own public; but it differed essentially (as he demonstrated by figuring it) from Pedalion. This creature, as to its classification, he would place simply among *Arthropoda*, hollow-limbed animals. What, broadly speaking, was the difference between a worm and an insect? It was the *homonomy* of one and the *heteronomy* of the other. Take a transverse section of a worm and you get the sections repeating themselves throughout the extent of the form; but in the transverse sections of insects you get heteronomy; so if Pedalion be divided transversely you can get no homonomy; and its structure he thought fitly entitled it to rank among the *Arthropoda*.

The President asked whether it was certain that there were no ciliated larvæ of Crustacea.

Mr. Stewart said he never heard of such a thing; he had examined a good many Crustacea, and had never seen larvæ.

A vote of thanks was then passed to Dr. Hudson.

The President then read a paper "On the Development of the Skull of the Crow."

Mr. Hogg wished to impress upon the Fellows that the paper which had been read to them was the result of many months' hard work at the microscope. The President took the embryo from the time that it was a day old, and watched its development day by day until the whole structure has been worked out. He merely mentioned the fact, as the President scarcely did himself justice by merely placing the crude results of his work before the Fellows. He (Mr. Hogg) had had the pleasure of seeing the President at his labours, and he could not help testifying to the wonderful patience and skill with which he conducted his observations. He concluded by moving a vote of thanks to the President.

The next ordinary meeting of the Society will be held on the first Wednesday in November, and a scientific evening for the exhibition of objects and conversation will be held on the 11th of December, particulars of which will be sent to the Fellows in the usual way.

Donations to the Library and Cabinet, from June 5th to Oct. 2nd, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Journal of the Society of Arts. Weekly	*Society.*
The Lens, No. 3	*Editors.*
Smithsonian Report for 1870	*Smithsonian Institution.*
Report of the Commissioner of Agriculture for 1870	*Ditto.*
Monthly Report Department of Agriculture for 1871	*Ditto.*
Gibson's Flora of Essex	*Author.*
Two Cases of Cancer. By J. J. Woodward	*Ditto.*
Popular Science Review, No. 44	*Editor.*
Journal of the Quekett Club, No. 19	*Club*
Quarterly Journal of the Geological Society, No. 111	*Society.*
Acta Universitatis Lundensis, 1869 and 1870	*University.*
The Canadian Journal, July, 1872	*Institute.*
Berichte des naturwissenschaftlich-medizinischen Vereines in Innsbruck, 1872	*Univ. Buchhandlung.*
Transactions of the Linnean Society, Vol. 28, Part II.; and Vol. 29, Part I.	*Society.*
Journal of the London Institution, No. 16	*Institution.*
Series of Photographs, showing the Resolution of Nobert's 19th Band with a Tolles' Lens. By Dr. Woodward	*Author.*

Walter B. Cole, Esq., was elected a Fellow of the Society.

Walter W. Reeves,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

September 27th.—Microscopical Meeting. Mr. G. Scott, President, in the chair.

Mr. Wonfor announced that the Lewes Natural History Society would hold a Microscopical Soirée on the following Monday, to which members of the Brighton Society were invited. He hoped some would be able to attend with their microscopes, and return the kindness rendered by the Lewes Society at the British Association Soirées.

Mr. Wonfor then remarked that Dr. Carpenter, F.R.S., very kindly gave him a small quantity of the Atlantic chalk dredged up in the Porcupine Expedition. Some of this he had brought down for distribution, and, if time allowed, he would show that it contained foraminifera identical with those obtained from the Chalk Downs.

Mr. Hennah then drew attention to a new series of objectives constructed by Mr. Wenham. Some would recollect seeing the new patent objectives at the Association Soirées; but Mr. Wenham kindly promised to send some down for exhibition to the Society. They were on a perfectly new method of construction, two or three of the lenses of the older construction being dispensed with, and a single front and back lens used. It was hoped thus to obtain a cheaper series of object-glasses; but that would be no advantage unless better performance was also secured.

Those sent down were all very good,—the high powers were especially so; but he would advise the members to wait for still greater improvements. The $\frac{1}{15}$th was an exquisite glass when properly adjusted, which, except in the hands of an experienced microscopist, was not easy of accomplishment. He considered the $\frac{1}{10}$th a perfect glass, with a good working distance. They could be used as dry or immersion lenses. At present there was an inequality, for, while very good as dry on some objects, they were not so good when wet, and *vice versâ;* but on some objects the performance was equally good, whether wet or dry.

He had written to Mr. Wenham on the subject of an objective with moderate angle of aperture for naturalists' work, because he did not believe in the high-angled glasses at present in use, and he would read what Mr. Wenham said:—"I agree with you that a $\frac{1}{20}$th of about 95°, with corrected aperture (I do not mean a large aperture *cut down* to 95°), but a system just worked up to that aperture, so as to have a long focal distance, say $\frac{1}{30}$th of an inch, would be a valuable glass in the hands of the naturalist, enabling him to *see into* things instead of a mere surface observation of a few diatoms, for the sake of performing the feat of defining the difficult marking of some half dozen of them, and this is only what such a glass is at present used for."

He considered this a wonderful admission, and any new system which would give them objectives for physiological and natural history investigations would be a great advance in the utility of the microscope. Another altogether perfect objective sent down for exhibition was a ½-inch. While the magnifying power was very great, the definition was clear and as deep as in an inch or inch-and-a-half. It was of 85° aperture, had a good working distance, was coned down to admit the pencil of light for bull's-eye or any other illumination, with a very large Lieberkuhn for opaque objects. There was also the patent reflex illuminator, for getting opaque illumination, with very high powers, which did its work remarkably well. Mr. Wenham, as an amateur, had done more than anyone else for the microscope; he had devised the binocular and given it to the world. While directing some improvements in the series of objectives of Messrs. Ross he had hit upon the form they have patented. These experiments have cost them a large sum, but Mr. Wenham had no pecuniary interest in the patent.

Mr. Wonfor inquired whether the higher objectives worked readily through ordinary covers.

Mr. Hennah replied that they did. He would exhibit a *P. angulatum* with a comparatively thick cover.

Votes of thanks were then given to Messrs. Hennah and Wenham. The meeting then became a conversazione, when Messrs. Hennah, Glaisyer, and Wonfor exhibited various objects with the new objectives. Their performance was pronounced admirable. The $\frac{1}{15}$th and $\frac{1}{10}$th showed *P. angulatum*, and other diatoms equally well, with Podura scales, and ultimate fibre from the pig, while the ½-inch gave beautiful definition and as great a depth as most 1-inch objectives. Later in the evening Mr. Hennah strained up the ½-inch with the highest eyepiece, and showed *P. formosum* marvellously.

Some very interesting objects were also exhibited by Messrs. F. E. Sawyer, W. H. Smith, and Shillingford; and Mr. Wonfor during the evening showed identical forms of foraminifera from chalk from the interior of a flint nodule picked up on the Downs and from *Atlantic chalk.*

READING MICROSCOPICAL SOCIETY.*

October 9th 1872.—Dr. Shettle presided; Captain Lang being unavoidably absent.

Mr. Tatem laid before the Society "Notes on new Acarelli," accompanied by drawings and balsam-mounted specimens of *Acarellus pulicis* and *Acarellus muscæ;* both being transitional forms which would doubtless have developed into perfect *Acari.*

Mr. Austin exhibited mycelium and spores of *Peronospora infestans*, with sections of sound and diseased potatoes, and of Dodder, showing the manner of its parasitic attachment to plants.

Mr. Tatem exhibited mounts of *Trombidium* and entire insects.

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* Reported by Mr. B. J. Austin, 107, Oxford Road, Reading.

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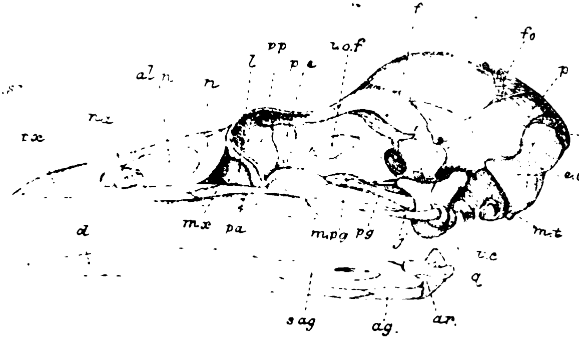
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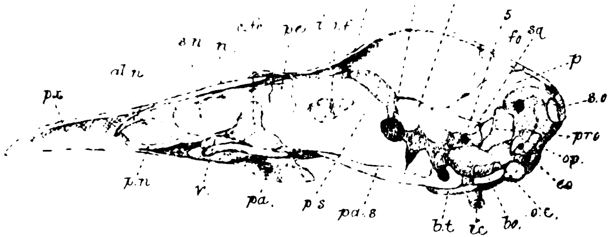
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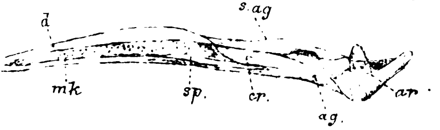
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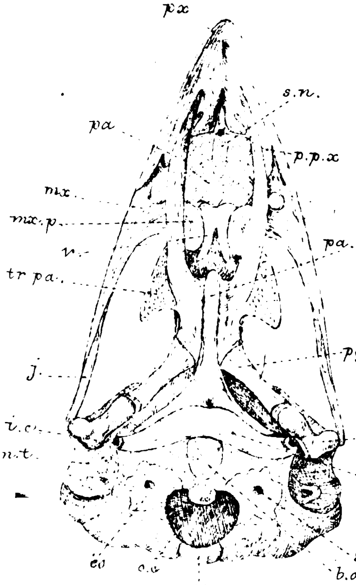
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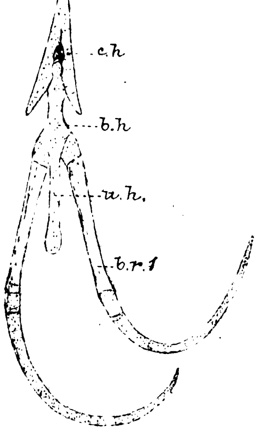
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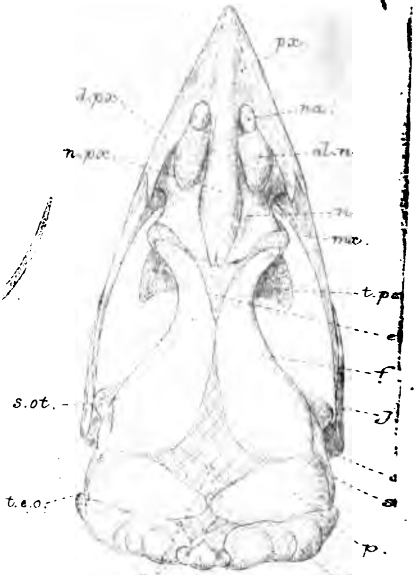
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THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1872.

I.—*Description of the Plates illustrating Mr. W. K. Parker's Paper on the Development of the Crow's Skull.*

(*Continued from page* 226.)

[N.B.—The description of the various stages of the Crow's Skull, and the first three Plates, will be found in the November number of this Journal, pages 217–226.]

DESCRIPTION OF PLATES.

Plate XXXVII.—Structure of the skull of the Nestling of the Carrion Crow (*Corvus corone*), one week old. The figures are enlarged one-half. Fig. 1. Basal view of skull and face. Fig. 2. Side view of skull and face, including the mandible, showing its outer aspect. Fig. 3. Upper view of the skull, upper jaw, and nasal sacs. Fig. 4. A longitudinally vertical section of skull and face, showing the brain-cavity, auditory bones, septum nasi, and cranio-facial axis. Fig. 5. End view of skull and lower jaw. Fig. 6. Inner view of right mandible, or lower jaw. Fig. 7. "Os hyoides," or distal part and base of 2nd post-oral arch, and the whole of the third, with its basal element.

Plate XXXVIII.—Figures illustrating the skull of a Fledged Nestling of the Rook (*Corvus frugilegus*), natural size. Fig. 1. Basal view of skull, face, and nasal capsules. Fig. 2. Side view of skull and face, including outer view of mandible. Fig. 3. Upper view of skull and upper jaw. Fig. 4. A longitudinally vertical section of skull and nasal capsules, showing cranial cavity, auditory mass, septum nasi, cranio-facial axis, and inner view of mandible.

Plate XXXIX.—Skull of Fledged Nestling of Rook—continued. Fig. 1. End view of skull of a scarcely fledged Nestling. Fig. 2. Ditto of an older Nestling. Fig. 3. Nasal labyrinth, dissected out and seen from above. Fig. 4. Transversely vertical section of nasal capsule in front of lateral ethmoids. Fig. 5. A similar section, seen behind the lateral ethmoids. Fig. 6. Inner view of nasal capsule, as seen in a longitudinally vertical section. Fig. 7. "Os hyoides."

II.—*Proposal for a Standard of Comparison of the Magnifying Powers of Compound Microscopes.* By J. E. Ingpen, Esq.

(*Read before the* Royal Microscopical Society, *Nov.* 6, 1872.)

Every worker with a telescope finds the necessity of ascertaining with considerable accuracy the magnifying powers of his instrument with its various eye-pieces. This is done by measuring the diameter of the reduced image of the object-glass in the eye-piece, when the telescope is focussed for parallel rays. The aperture of the object-glass, divided by the diameter of its reduced image, gives the magnifying power. The advantage of this method is, as

Admiral Smyth says, that "it does not require any knowledge of the thickness and focal lengths of any of the lenses employed in the telescope, nor yet of their number and relative positions;" and he quotes Dr. Pearson thus: "One operation includes the result arising from the most complicated construction, and sets theory at defiance with respect to calculations that must take into consideration the previous determination of all the preceding requisites, the obtaining of which is attended with practical difficulties almost insurmountable."* I can hardly imagine but that some one interested both in telescopic and microscopic research must have long since applied the same method to the microscope that has been used for the telescope with perfect success for more than a century past. So far as I know, however, this has not been done, or at any rate, published, and I therefore venture to bring this matter before the Royal Microscopical Society, in the hope that it may be of interest and utility to some few amongst us, and perhaps lead to further inquiry.

The conditions under which we use this plan of obtaining the magnifying power are not quite similar in the two cases. In the astronomical telescope we deal with parallel rays, and the focus is therefore constant for each instrument: in the microscope we use various objectives as well as eye-pieces, and conjugate foci instead of parallel rays. But if we fix upon a standard object and view it at a known distance the method is equally applicable. The standard I propose to use is the diminished image of a disk one inch in diameter, placed at ten inches below the position of an object upon the stage of the microscope; then the real diameter of that disk divided by the diameter of its reduced image will give the standard magnifying power with the objective eye-piece and length of body employed, and if the length of body is such as to make the distance between the front surface of the objective and the diaphragm in the eye-piece *ten inches*, then the real as well as the standard magnifying power is given.

The reduced image of a disk one inch in diameter viewed at ten inches below the stage is too small to be measured with accuracy when high powers and deep eye-pieces are used, but any multiple of one inch or divisor of ten inches can be employed. For most purposes the following plan seems to answer: A revolving diaphragm, capable of being adapted to the stage, is pierced with apertures of ¼ inch, ½ inch, and 1¼ inch, and fixed to a tube at 2½ inches from a plate of thin glass, upon the upper surface of which the objective can be focussed. This will allow of the objective being used at "uncovered," at "covered," with any known thickness of covering glass, and with a wet or dry front. On the same, or another diaphragm, some minute holes, at known distances apart, such as ½ inch and 1¼ inch, can be pierced, and used in the same manner. It is evident

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* Smith's 'Celestial Cycle,' i., 382.

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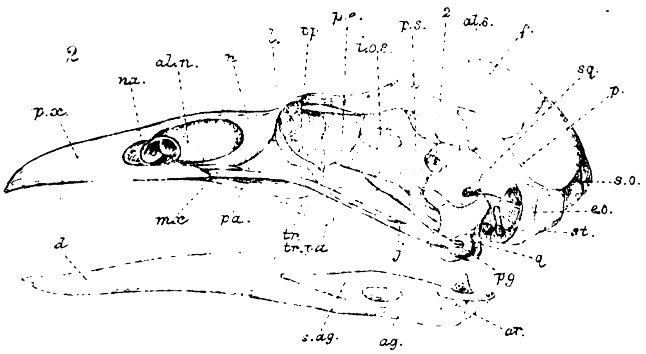

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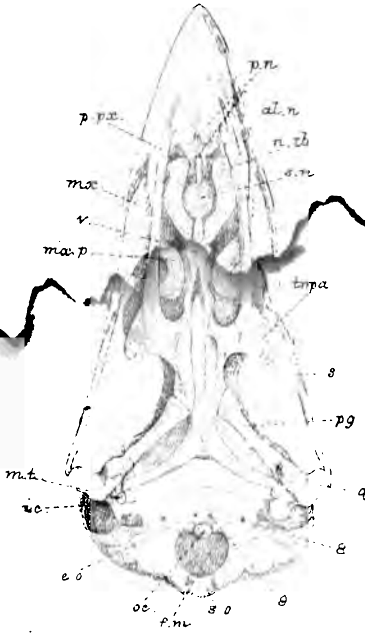

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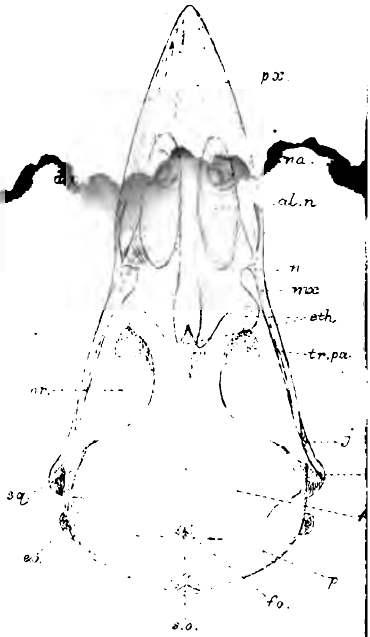

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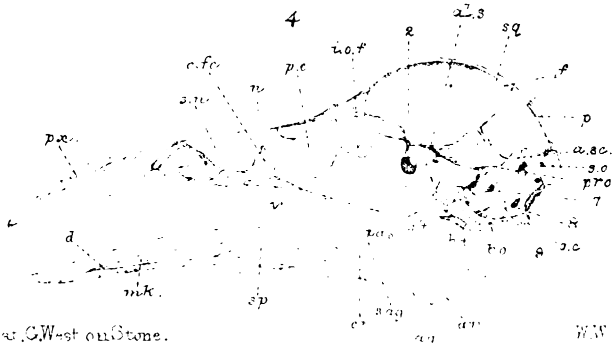

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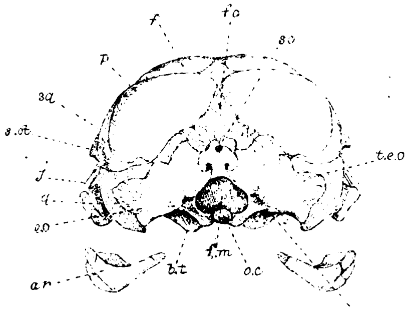

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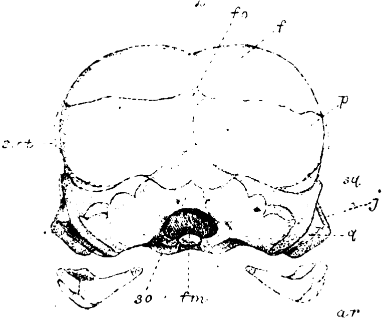

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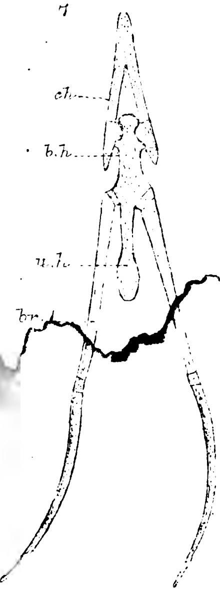

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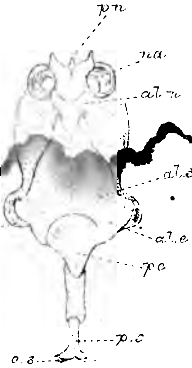

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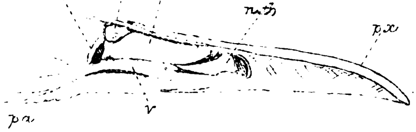

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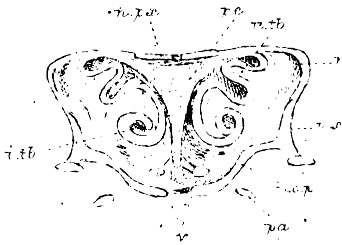

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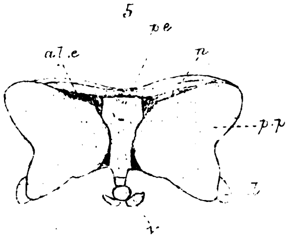

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that the ¼-inch aperture, or the holes ¼ inch apart, represent the 1-inch disk, the ½-inch aperture a 2-inch disk, and the 1¼-inch aperture a 5-inch disk at ten inches distance. In the latter cases the result would have to be divided by 2 or 5 to obtain the power.

The next point is as to the mode of measuring the reduced image in the eye-piece. For low powers the ordinary divided scale dynamometer may suffice; for higher powers a double-image dynamometer, or one with a compound microscope, would be necessary. With high powers a measurement with the lowest eye-piece would suffice, as the *relative* powers of the eye-pieces can be very exactly obtained: the mean of several observations should be taken. It may be as well to subjoin an example to show the method of working:—

Disk 1¼-inch at 2½ inches = 5-inch disk at 10 inches distance.
Diameter of image ·028) 5·00 (178·6 power nearly.

I venture to think that this method will be found well adapted for a standard of measurement. It is simple, showing the power with the smallest amount of calculation, whatever may be the foci and construction of the objective and eye-piece, and length of body, adjustment for cover, &c. If the body is made, as before suggested (by means of suitable adapters), to measure ten inches from the front surface of *each* objective to the stop in *each* eye-piece, the standard will agree with the real measurement; and even if the difficulty of measuring the minute disks of light be considered too great with high-power objectives, even approximations may be useful, if based upon a standard the conditions of which can be easily remembered and reproduced.

There are two uses of the *optical* portion of the dynamometer, which, though long known, seem to have been too much overlooked of late years. In the form of Ross's centreing eye-piece, *viz.* a positive eye-piece of moderate power, means are afforded of ascertaining very exactly the condition of the inside of the tube, the positions of stops, existence of stray light, extent and centricity or obliquity and direction of illumination, &c. The other use is that of obtaining the *real working* angle of aperture of an objective when direct light is used and the eye-piece is in position, and not merely the angle at which a burr of oblique light may be *just* visible. This plan was used in 1862 in examining the objectives at the International Exhibition.*

If a double-image dynamometer is used, it must be ascertained if astigmatism exists in the eye of the observer. This defect (which is much more common than is generally supposed) will greatly vitiate the measurements, unless corrected by the use of proper (cylindrical) lenses.

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* See 'Quarterly Journal of Microscopical Science,' IV., N.S., 1864, p. 84.

III.—*On the Structure of the Valves of Eupodiscus Argus and Isthmia enervis, showing that their silicious deposit conforms to the general plan of deposition in simpler forms.* By HENRY J. SLACK, F.G.S., Sec. R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Nov.* 6, 1872.)

PLATE XL. (Middle portion).

THE observations now brought before the Royal Microscopical Society may be regarded as a continuation of those previously communicated on Pinnulariæ, as they have the same object, and tend towards the same conclusion that the silicious deposition in diatoms probably follows one uniform plan, and that the silex is deposited in spherules, varying in dimensions and in degree of proximity; thus according with Mr. Wenham's researches.

Since the publication of the paper on Pinnulariæ numerous fresh observations have been made, confirming the results then mentioned, and efforts were occasionally directed towards elucidating the structure of such forms as *Isthmia enervis* and *Eupodiscus Argus*, and showing their agreement with the ordinary types; but without satisfactory results so long as only old means were employed.

Lately, with the help of Mr. Wenham's "Improved Reflex Illuminator," described in a paper read before this Society last May, and published in the June number of this Journal, the markings of *Eupodiscus Argus* have been so clearly displayed as to leave little doubt of the real structure, and those of *Isthmia enervis* have been exhibited so as to indicate that the aberrant character shown under ordinary illumination is the result of a peculiar arrangement of minute beads viewed under circumstances of optical confusion.

No thoughtful microscopist could be satisfied with the views *Eupodiscus Argus* obtained by ordinary means, and corresponding in general effect with the figure given in the 'Micrographic Dictionary,' pl. 12. In Smith's work on the Diatomaceæ a better figure was given, but far from being right, though at the time of its publication there was probably no particular reason known for doubting its correctness.

The figures in the 'Micrographic Dictionary' (2nd edition) would suggest to anyone familiar with the confusion arising from defective illumination and the superposition of transparent surfaces that it must be wrong. It represents a number of bright objects something like white stars with short rays, on a dark ground of general confusion. In the text of the same work the genus Eupodiscus is described as having valves that "appear either distinctly areolar, the depressions being large: granular from their being minute; or striated."* In the last edition of 'Pritchard's Infusoria' we are told that *Eupodiscus Argus* is "easily recognized by its irregular cellules and intervening puncta, which give the disk a clouded appearance very unlike the usual transparency of Diatomaceæ."

Until Mr. Wenham's researches settled the old disputes † as to whether the markings on ordinary diatoms were elevations or depressions such phraseology as the above might be admissible, but careful examination of the objects with the best optical means now at the command of the microscopist may be expected to banish such terms as "areolar," "cellules," &c., from the descriptions of diatoms; the so-called "cellules" being spherules of silica, and the "areolæ" (*little spaces*)—not at all corresponding with what is generally meant by such a term.

At the end of Mr. Wenham's paper on the new illuminator, he remarks that "in English-mounted slides of diatoms and insect scales, some are always found detached from the cover and on the slide, and are shown well with it; but such is not the case with Möller's and others: they raise their covers off the slides by a ring of cement, and in many of them, I have searched in vain for a solitary specimen that has become detached." In my specimen of Möller's type slide some diatoms were found that could be well exhibited by Mr. Wenham's plan, though not by the same action of his instrument as when dry slides are used. Among these were the *Eupodisci* placed at the corners of each series. This result was not, however, arrived at without a good deal of trouble and practice in using the apparatus, and microscopists who take it up for the first time must not be disappointed if they frequently fail in its employment until they have spent a good many evenings in testing its action at various points of rotation, with the sub-stage mirror throwing up the light, and at various angles.

When successfully employed upon *Eupodiscus Argus* a considerable portion of the circular valve will become clear and plain, and it will be seen that it is entirely composed of spherules of different sizes and varied aggregation. Radiating from a central portion, occupied by minute and closely-packed spherules, bands will be seen proceeding to the circumference, each one composed of minute spherules that appear in close contact under an ⅛, with eye-pieces up to D of Ross' system. Between these bands are larger spherules, frequently, but by no means universally, arranged in *fours*, so that when seen with powers too low for their distinct separation they appear to form patterns like the Gothic quatrefoils, and look like holes when the light passes straight through them.

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* In the edition of the 'Mic. Dic.' now issuing in numbers, pl. 12, an additional figure is given of part of a valve of *E. Argus* a little nearer the mark, but far from being right.

† This was written, not anticipating Mr. Stewart's remarks in discussing this paper. The exact refractive power of various diatoms—for they seem to differ—should be ascertained.

To see this structure well the mirror must be inclined so that the light given by Mr. Wenham's apparatus all passes a little askew through the spherules. When, through refraction or reflexion, any light passes straight, or nearly straight, through the larger spherules, they look like holes through which a glare penetrates, and all minutiæ of structure disappear. A slight change in the angle of the mirror, or of the position of the Wenham illuminator effected by partial rotation, will leave the larger spherules in a quiet glow, as if they were incompletely transparent, while the smaller spherules of the radial bands glisten a pearly lustre. A ⅕th of Beck's with a deep eye-piece is sufficient to show this structure, but an ⅛th will do it better, and an excellent effect was obtained with Powell and Lealand's new pattern and dry front.

Good specimens of *Eupodiscus Argus* should be examined as uncovered objects both with Mr. Wenham's apparatus and with that devised for opaque objects under high powers by Professor Smith. The writer has hitherto had no opportunity of trying these plans.

With regard to *Isthmia enervis*, the 'Micrographic Dictionary' describes the valves as "without linear thickenings, uniformly covered with depressions," and the valves of the genus generally are spoken of as having "depressions so large as to produce a distinct reticular or cellular appearance when viewed by ordinary illumination." This last remark is quite correct, and the "distinctness" of the appearance has caused it to pass unquestioned.

In a last edition of 'Pritchard's Infusoria' the valves of *Isthmia enervis* are described as having "large, somewhat quadrate cellules," which corresponds with the appearances ordinarily seen. In the same work, at page 42, is a quotation from 'Silliman's Journal,' describing experiments of Professor Bailey, who viewed diatom valves with the microscope while they were being acted on with dilute hydrofluoric acid. "In Isthmia," he said, "the spots on the surface which appear like granular projections, are in reality thin portions of the shell, and under the action of the acid they soon become holes." It is obvious that the parts which dissolve first must either be the thinnest or so constructed as to present the greatest surface to the acid. Professor Bailey's observations must therefore guide us in deciding which of several appearances the valves can be made to assume under high powers and various modes of illumination.

The writer has not yet been fortunate enough to find specimens of Isthmia so situated as to show in the best manner with Mr. Wenham's illuminator, but with a fine ⅛th objective and that apparatus the simple cellular or reticulated appearance disappears, and great numbers of minute spherules at different levels can be discerned; and its structure is thus seen to conform to ordinary types.

In Plate XL., a portion of the valve of *Eupodiscus Argus* is shown as seen in the manner described. No figure of Isthmia is given, because, though enough was seen to justify the preceding remarks, some important points in the arrangement of the minute spherules were left in doubt.

IV.—*Notes on the Development of the Nervous System of the Annulosa.* By B. T. Lowne, M.R.C.S., F.L.S., Lecturer on Physiology at Middlesex Hospital Medical School.

(*Read before the* Royal Microscopical Society, *Nov.* 6, 1872.)

Plate XL. (Lower portion).

A year and a half ago (January 11, 1871) I had the honour of reading a paper before you "On the Anatomy of *Ascaris lumbricoides*," and I then ventured to express the opinion that the nervous system of the Invertebrata is developed, like that of the Vertebrata, from the serous layer of the blastoderm. I had at that time set myself the task of working out the subject, but time has not sufficed to do what I had wished, and as a year or more may yet elapse before I have worked out this interesting matter, I have been induced to give you a *résumé* of what is at present known on this head, and to furnish you with the principal facts which have led me to believe that such is really the case.

In June, 1871, Dr. E. Metschnikoff published an essay in Kölliker's 'Zeitschrift' on the development of scorpions, which has confirmed my opinion, as his observations are a connecting link between my own as yet imperfect researches. This essay changed what was little more than suspicion in my mind into a strong belief.

DESCRIPTION OF PLATE XL. (Lower portion).

Fig. 1.—The ventral surface of an embryonic scorpion, showing the chain of ganglia, *a a*, on either side of the primitive furrow, one being situated at the base of each appendage. After Metschnikoff.

„ 2.—A transverse section through one of the ganglia and lateral appendages of a younger embryo. *a*, ganglion in the epiblast; *b*, rudiment of a leg outside the ganglion; *c*, mesoblast; *d*, hypoblast. After Metschnikoff.

„ 3.—The extremity of the abdomen of a scorpion embryo, showing the splitting of the mesoblast to form the body cavity. *a'*, epiblast; *c*, mesoblast; *d*, hypoblast; *e*, body cavity. After Metschnikoff.

„ 4.—A section of the entothoracic epiphysis of the mesothorax of a fly, showing its relation to the nerve ganglia. *a*, process of integument; *b*, nerve centre bound down to the thorax; *c*, nerves passing from it.

I cannot do better than give you a brief account of what Dr. Metschnikoff has done, before passing to the more general consideration of the subject which I have in view.

According to the observations of that author, the first trace of the embryo scorpion is a longitudinal groove in the blastoderm: this groove afterwards disappears for a time, but subsequently becomes visible again as a deep groove, on either side of which the nerve ganglia of the ventral chain appear as thickenings of the external layer of the blastoderm. Nerve fibres are next seen penetrating the median layer, or mesoblast. The mesoblast splits, as in vertebrates, to form the body cavity, its inner layer becoming incorporated with the inner or visceral layer to form the alimentary canal.

In connection with these observations, the remarkable internal skin growths which support the principal nerve centres of insects and crustaceans are of the highest significance; as it is quite clear that they are the highly-developed remains of the primitive fold in which the nervous system is formed. Fig. 4 is a section of one from the thorax of the fly, and it shows the close relation of the nervous and cutaneous systems.

One of the most striking circumstances in the development of the nervous system is the bifurcation of the neural groove and chain at its anterior or cephalic end, so that it embraces the mouth and pharynx in a kind of fork, the anterior extremity of which gives origin to the great preœsophageal or prestomal ganglia.

The ganglia of the Articulata are chiefly developed at the bases of the lateral appendages; the rudimentary legs, antennæ, &c., may be conveniently divided into prestomal and poststomal ganglia. The prestomal nerve centres, like the brain of the vertebrate embryo, make a hæmal curve, which is most decided in the most highly developed forms, both in the Crustacea and the Insecta. The hæmal curvature of these nerve centres was pointed out by Professor Huxley as early as 1857 in the following terms:—"It is exceedingly interesting to remark the correspondence between the embryonic structure of the head of Mysis (and, I may add, of the other Articulata) and that of the vertebrate embryo. The procephalic processes resemble in a remarkable manner the lateral cranial processes of the vertebrate embryo; and the cephalic flexure of the crustacean, or insect, has its analogue, if not its homologue, in the cranio-facial inflection of the higher Vertebrata."*

Dr. Weissmann, in his 'Embryology of the Diptera,' has described the same primitive ventral groove in the early embryo of the fly, and my own observations on the development of the blow-fly not only confirm Dr. Weissmann's, but they go far to establish the fact that in the highest and most modified forms of the Insecta the development of the nervous system takes place in the serous layer of the blastoderm. As yet a complete series of investigations are wanting, but I have traced the steps of development sufficiently to allow me to state that the great procephalic lobes which exist in the half-developed embryo become folded inwards, and lie one on either side of the alimentary canal during the whole period of larval life, and that the nervous system of the larva, by the modification of which that of the imago is developed, is formed as a thickening of that portion of them which originally bounds the primitive fork at the anterior extremity of the ventral groove. These involuted procephalic lobes—and they are nothing else—form a portion of the imaginal disks of Weissmann, whilst the eyes, antennæ, and mouth-organs, are ultimately developed from cellular outgrowths at the bases of the same structures, just as they are in the Crustacea.

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* Lectures on Gen. Zool., 'Med. Times and Gaz.,' 1857, p. 638.

The nerve centres of the higher insects, such as the blow-fly and the higher social Hymenoptera, are even more remarkably like those of the Vertebrata, for a pair of cerebroid ganglia are developed in them in the most anterior portion of the procephalic lobes. These cerebroid ganglia were first pointed out in the Hymenoptera by Dujardin.* The ganglia in question are pedunculated convoluted nerve centres, situated at the anterior and upper part of the pre-stomal nerve centres; they are united by a well-marked commissure, never give off nerves directly, and are further connected with the great ganglia of sensation by peduncles, just as the cerebrum is in the Vertebrata.

At present I have not, it is true, succeeded in tracing the development of these cerebroid ganglia, but do not doubt I shall ultimately do so. From their position it is not difficult to see that they must originate from the anterior (in after-development posterior by flexion) portion of the procephalic lobes.

Having thus pointed out to you very briefly the present condition of research as to the relation of the nervous system of the Arthropoda and Vertebrata, I will draw your attention to the relation of the nervous system of the higher Annulosa to that of the Annelida and Annuloida generally.

The nervous system of the Echinodermata will serve well as a starting point. It consists, as you well know, of a band of nerve tissue surrounding the pharynx, and composed of nerve fibres with nerve cells scattered amongst them. From this circular ganglion band radiating ganglion bands (usually five in number), having the same structure, pass with the ambulacral vessels towards the antambulacral, or, as it is sometimes called, dorsal region, that is, towards the anal pole of the animal.

If I may be allowed to speculate on this subject, it seems to me probable that the primitive nerve ring is connected with that involution of the dorsal integument of the embryo from which the ambulacral system is developed, and is hence derived from an external layer of the embryo. I throw this out, however, as a mere suggestion.

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* 'Ann. Sc. Nat.,' Series iii., T. xviii., p. 231.

The next type of nervous system to which I would draw your attention is common amongst the Scolecida, especially those which, like the Nematoids and Gordiaceæ, bear many resemblances to the Echinodermata. The nervous system consists of a ring of nerve tissue around the pharynx, histologically like that of an Echinoderm, and closely related to the cellular layer of the integument, of which I believe it to be a differentiation. From this two or more bands of similar structure pass, according to my own observations,* to the anal pole of the body. This seems to be the case in the Ascaris at least, where I have traced short rudimentary intermediate bands passing back from the oral ring between the main nerve bands; these apparently represent the three missing radiating nerves of the typical Echinoderm. Some forms, as Mermis, have three instead of two well-developed nerves passing back from the œsophageal ring.

In the earth-worm, according to the elaborate researches of E. Claparède (whose much-lamented death is one of the greatest losses zoology has met for years), the structure of the nervous system follows a precisely similar type. In this case, however, only two bands pass back from the œsophageal nerve ring, and these are closely united and lie in the middle line on the ventral aspect of the body. Histologically, their structure is nearly like that of the Echinodermata, but the nerve cells show a tendency to become aggregated in places; this is the first indication of the formation of distinct ganglia.

The transition from this condition to that observed in the higher Annelida, Insecta, and Crustacea, is not difficult. The nerve cells become collected in separate groups, or ganglia, and the remaining portions of the cords form the longitudinal commissures between them. The nervous system also becomes more central by the folding inward of the serous layer of the blastoderm to form the internal skin growths already mentioned. Thus we arrive at the highest forms, and we trace connecting links between the whole great series of organisms grouped under the sub-kingdoms Annuloida and Annulosa.

Before concluding, I cannot help referring to the visceral system of nerves so well marked in many Articulates, which is clearly developed from the mesoblast, as the sympathetic system is in the Vertebrata. I would draw attention to the great similarity of the complex visceral plexuses and the chain of visceral ganglia in the two great sub-kingdoms. These latter are undoubtedly differently placed, but their development from the mesoblast is a strong indication that they are to some extent homologous as well as analogous systems.

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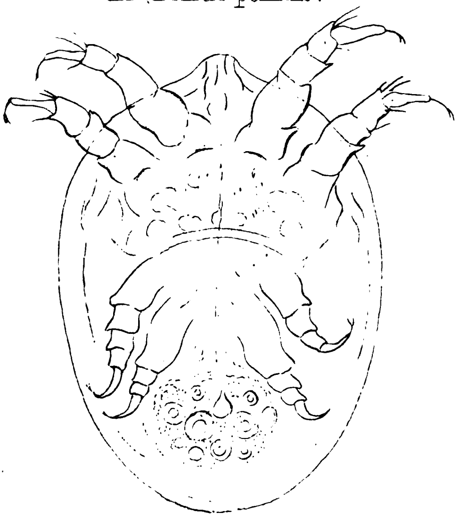
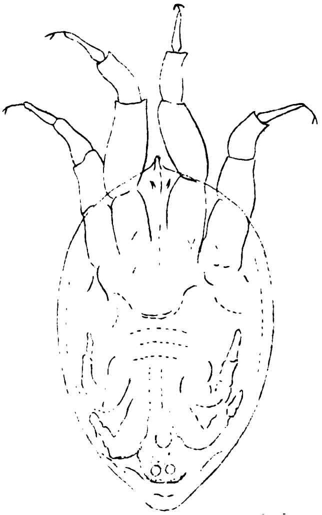
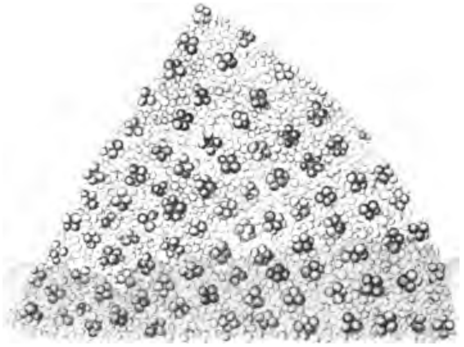
* 'Monthly Microscopical Journal,' January, 1871.

Acarellus pulicis.



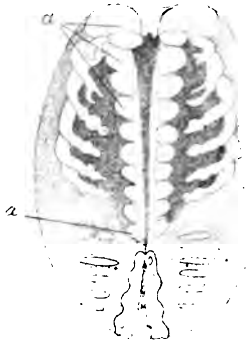
Acarellus muscæ.



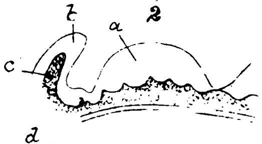


Portion of Valve of Eupodiscus Argus.

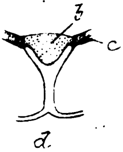
1



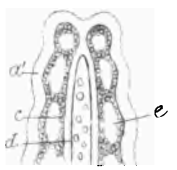
2



4



3



Development of Nervous System in Arthropoda.

W. West & Co. lith.

V.—*Notes on New Acarelli.* By J. G. Tatem.

Plate XL. (Upper portion).

In exhibiting balsam-mounted slides of new species of Acari, I wish to call the attention of the members to a fact of great significance in the life history of the creatures, and one, too, of much value generically considered. We all know that the young of the Acarine division of the Arachnid family are excluded from the ovum in an immature condition, possessing but three pairs of legs, though they acquire the normal four pairs after one or more subsequent moults. The common harvest bug, of which the adult form has yet to be recognized, is but a too familiar example of this hexapod stage of Acarine existence. The subjects I have embalmed have, however, entered upon the mite world in a still more imperfect state—they have but two pairs of legs!—but to prove that they would ultimately be as capable as any of their congeners of exercising four pairs, they have two posterior pairs neatly packed up in their trunks ready for evolution in the progress of growth!

Two years since, at the July meeting of the Entomological Society, Professor Westwood gave descriptions of two species of minute four-legged Acari, the one detected in the unopened buds of black currant trees, the other in pustules on the leaves of pear trees, referring also to a third which had been previously found in France. He considers them a distinct tribe, and that they must at all events be placed in a separate genus, for which he proposed the name of *Acarellus*. To this group our specimens unquestionably belong, and although their obviously imperfect development more than justifies the suspicion that the genus is based on immature forms alone, it still constitutes a good and natural division of the family.

In that example to which I have ventured to assign the specific name of *A. pulicis*, Plate XL. (its habitat having been the abdominal cavity of a dead flea), the hinder pairs of limbs are almost perfectly and completely formed, but still included within the cephalo-thorax, evidently awaiting the nearly approaching moult for extrication; while in the other, which on like grounds I have called *A. muscæ*, Plate XL. (detected clinging tenaciously in company with half-a-dozen others to the thorax of a small dipteron), the hinder pairs are more rudimentary, and with its much smaller size indicates an earlier stage of existence and more remote ecdysis. In both instances, however, it is quite apparent that in the process of development they would sooner or later have transferred themselves from the new genus *Acarellus* to that of *Acarus* proper.

Of their life histories I have no knowledge. Discovered by accident, and not until after many hours of maceration in liq. potassæ, I have yet to gain the opportunity of observing them in life; and though, as I suspect, the species may be numerous and examples abundant, minuteness ($\frac{1}{120}$ to $\frac{1}{100}$) and parasitism will oppose so many obstacles to the inquiry, that by vigilant and patient research alone may we hope to gain any further acquaintance with them.

VI.—*On a Proposed Standard Micrometer Eye-piece, or Eidometer; and on a uniform and easily applied Method of Naming Microscopic Objectives.* By George Findley.

With a proper micrometer eye-piece, nothing can be easier than to *see* the exact magnifying power of any microscopic objective[*] at any interval of object and image, and of naming it accordingly.

I have had one of my own adapting in use for over six months, calculated according to the usual formula for eye-pieces, $\frac{f \times e}{f + e - d}$; where f is the focal length of the field-lens; e, that of the eye-lens; and d is the distance between them. I think it would help to settle some of the disputes among microscopists as to the solar focal length of objectives. Some of the uses it may be put to are the following:—

1. Place on the stage of the microscope a micrometer ruled to $\frac{1}{100}$ths and $\frac{1}{1000}$ths of an inch, and the eye-piece shows at sight how many $\frac{1}{100}$ths or $\frac{1}{1000}$ths in the eye-piece micrometer are covered by the image of $\frac{1}{100}$th or $\frac{1}{1000}$th on the stage micrometer. The size of the object $\frac{1}{100}$th or $\frac{1}{1000}$th is known; the objective makes a magnified image of it, which is measured by the eye-piece micrometer. The ratio of the size of the *seen and measured magnified image*, to the size of the known unmagnified object, gives the magnifying power of the objective. If there be a draw-tube, there will be shown the increase of size given by the increase of length of tube.

2. My eye-piece of itself magnifies the image just 10 times, making the total magnifying power with it, just 10 times that of the objective alone.

3. It furnishes us with the means of knowing the magnifying power of any other eye-piece.

4. It enables us to tell the *real size* of the original object.

5. It enables us to calculate the principal focus of the objective, from the seen magnifying power and the interval of object and image, *i. e.* the distance of the conjugate foci from each other.

6. Its power to magnify 10 times may be shown to the eye.

7. It renders practicable the method of naming objectives according to their *seen magnifying* power at a standard distance, say

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[*] Simple or compound, dry or wet, object covered or uncovered.

10 inches,* of object and image, or of the two micrometers. If an objective is seen to magnify at the standard distance 5, 10, or 20 times, call it objective No. 5, 10, or 20, or O_5, O_{10}, O_{20}.

8. It also renders practicable the method of naming eye-pieces according to their real magnifying power. Thus, if the micrometer eye-piece makes $\frac{1}{10}$th or $\frac{10}{100}$ths in the eye-piece micrometer cover 1 inch on a rule at 10 inches from the naked eye, call it eye-piece No. 10, or E_{10}. Then since in the compound microscope the whole power (M) is the power of the objective (O) × the power of the eye-piece (E), we have

$$O \times E = M,$$
$$O = \frac{M}{E},$$
$$E = \frac{M}{O},$$

or

$$\begin{aligned} O_5 \times E_{10} &= M\ 50, \\ O_{10} \times E_{10} &= M\,100, \\ O_{10} \times E_{20} &= M\,200, \\ O_{40} \times E_{10} &= M\,400, \\ O_{40} \times E_{20} &= M\,800, \text{ \&c., \&c.} \end{aligned}$$

Any two of these three (O, E, M) being given, the third is easily found.

9. With this eye-piece there need be no disputes as to whether a certain objective be $\frac{1}{8}$th or $\frac{1}{10}$th, or whether a $\frac{1}{16}$th be or be not $\frac{1}{19}$th, since it shows us how much it magnifies; and here "seeing is believing."

It would also save Dr. Royston-Pigott *and others* the trouble of taking the glasses out of their eye-pieces; placing a micrometer in the empty eye-piece and examining it with a 1-inch lens;† and also Dr. J. J. Woodward the trouble of having to use a dark room, a screen, and direct sunlight, to get the magnifying power of his objectives, in order to calculate their solar or principal focal lengths.‡

I do not pretend to a totally new invention; but only to a new adaptation of an already existing instrument whose many uses in this direction seem to have been lost sight of.

If not out of place, I should like to say that though not indebted to Dr. Royston-Pigott for my eye-piece (which grew out of my own wants), I am very much indebted for various useful hints scattered

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* I prefer 10 inches; and one reason is that, *at* 10 *inches*, the real magnifying power is just 2 (and a fraction which may almost always be neglected) less than the nominal magnifying power.

Focus	1	in., Nominal power	10	times, Real power	8	times nearly.
„	$\frac{1}{2}$	„ „	20	„ „	18	„
„	$\frac{1}{3}$	„ „	30	„ „	28	„
„	$\frac{1}{10}$	„ „	100	„ „	98	„

and conversely the seen real power + 2 = nominal (within the before-mentioned fraction).

† 'Quart. Jour. Mic. Sci.,' July 1, 1872, p. 270.

‡ 'Mon. Mic. Jour.,' Aug. 1, 1872, p. 72.

through those published articles of his which I have seen. He teaches much, and suggests more.

The use of this eye-piece is not confined to the microscope. It will measure the size of the image in the focus of the objective of the telescope; also the breadth of the pencil emergent through any telescopic eye-piece; and so lead to the magnifying power of the telescope.

It also measures the distance apart of the Fraünhofer lines in the spectroscope; and, of course, would show the relative dispersive power of any prism or combination of prisms.

VII.—*On a Method of ascertaining Magnifying Power and Minute Magnitudes by Miniatures.*

By Dr. ROYSTON-PIGOTT, M.A., M.D. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., M.R.I., F.R.M.S., late Fellow of St. Peter's College, Cambridge.

THAT quaint old writer Dr. Kitchener says:—"An easy way of judging of the relative magnifying powers of various eye-tubes and lenses is to hold them up about 12 or 18 inches from the eye, and three or four feet from a window—that in which the panes of glass appear least, magnifies most: or place two candles nearly together and hold the lenses about a foot from them, that in which the candles appear nearest together magnifies most."

Upon this principle I now propose the following simple method of ascertaining the magnifying power of an object-glass, which *I call the miniature method.*

Place the object-glass in the position of a condenser. At exactly ten inches from the stage adjust a scale of inches and tenths. Bring the image of the scale upon a ground-glass slide, with its ground surface downwards. The ground glass now shows a miniature of the scale. Now replace it by a micrometer slide, and viewing it *with a low power,*[*] readjust the condensing object-glass so as to bring the image of the scale into a focus accurately coincident with the divisions of the stage micrometer. The actual size of the miniature scale can be read off upon the stage micrometer, which immediately gives the amount of diminution, and therefore of the enlargement.

Ex. (1). A micrometer on stage showed a tenth on scale miniatured by an object-glass to be 19 thousandths. The magnifying power therefore

$$= \frac{1}{10} \div \frac{19}{1000}$$
$$= 100 \div 19 = 5{\cdot}26.$$

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[*] A power of 100 is convenient.

If now this object-glass be employed at 10 inches, with an eyepiece C magnifying 10 times, the magnifying power will be 10 times increased, or 52·6.

Ex. (2). Suppose it be required to find the size of an image formed at 15 inches instead of 10, by a lens nearly $\frac{1}{20}$th of an inch focal length, which magnifies 200 times at 10 inches.

Then first the real focal length (see footnote) $f = \frac{d}{m+2}$

$$= \frac{10}{200+2} = \frac{5}{101}.$$

Then the magnifying power at 15 inches will be

$$15 \text{ divided by } \frac{5}{101} \text{ less } 2 = \frac{15 \times 101}{5} - 2$$
$$= 301 - 2$$
$$= 299.$$

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[*] A question often arises such as the following —If a given lens magnifies 20 times at 5 inches, what would it magnify at 10? Now this will not be just double. The law of images is somewhat complicated, and I find that if (d) the distance between the image and object (say 10 inches) be divided by the focal length (f), it will for *a single small lens* give the magnifying power m too great by 2. But for a lens of considerable focal length, such as $\frac{1}{2}$ inch, m = new distance divided by focal length less 2, to be further diminished by reciprocal of this result, which will afford a closer approximate value. Those who desire to see the solution of the problem will be able to follow the following equation, given by the writer, 'Phil. Trans.,' II., 1870 (p. 594):—

$$m + 2 + \frac{1}{m} = \frac{d}{f}, \left(\text{or nearly } m = \frac{d}{f} - 2\right) \qquad \text{(I.)}$$

$$m^2 + 2m + 1 = \frac{md}{f}$$

$$m^2 - m\left(\frac{d}{f} - 2\right) + 1 = 0.$$

Completing square,

$$m^2 - m\left(\frac{d}{f} - 2\right) + \frac{1}{4}\left(\frac{d}{f} - 2\right)^2 = \left(\frac{d}{f} - 2\right)^2 - 1.$$

Taking the root (positive sign),

$$m = \frac{1}{2}\left(\frac{d}{f} - 2\right) + \sqrt{\frac{1}{4}\left(\frac{d}{f} - 2\right)^2 - 1}. \qquad \text{(II.)}$$

The formula will give the magnifying power, in the same way for a distance $d = 20$ inches between object and image; for a focal length 1·8 as found in *Ex.* (3): which is a satisfactory application of the principle of verification:—

Thus, since $d = 20$ and f now $= 1\cdot8$,

$$m = \frac{1}{2}\left(\frac{20}{1\cdot8} - 2\right) + \sqrt{\left[\frac{1}{2}\left(\frac{20}{1\cdot8} - 2\right)\right]^2 - 1}$$

$$= \frac{16\cdot4}{3\cdot6} + \sqrt{\left(\frac{16\cdot4}{3\cdot6}\right)^2 - 1}$$

$$= \frac{16\cdot4}{3\cdot6} + \sqrt{\frac{256}{(3\cdot6)^2}} = \frac{16\cdot4}{3\cdot6} + \frac{16}{3\cdot6} = \frac{32\cdot4}{3\cdot6} = 9.$$

If the lens be nearly 2 inches focus, and magnifies 9 times at 20 inches, this method will not be sufficiently correct for so large a lens. In order to find its focal length, the distance of the "centre" of the lens from object and image is generally required; but my formula obviates this difficulty.

Ex. (3). To find the magnifying power at 36 inches of a supposed 2-inch lens, which magnifies 9 times at 20 inches distance between object and image:

First approximation, ('Phil. Tr.' *loc. cit.*)

$$m = \text{distance} \div \text{focal length less } 2 = \frac{d}{f} - 2.$$

But focal length (f), since $m + 2 + \frac{1}{m} = \frac{d}{f}$

$$= \text{distance divided by } \left(m + 2 + \frac{1}{m}\right)$$

$$= 20 \div \left(9 + 2 + \frac{1}{9}\right) = 20 \div \frac{99 + 1}{9}$$

$$= \frac{180}{100} = 1 \cdot 8 = f.$$

Then

$$m = \text{new distance} \div \text{focal length less 2 (approximately)}$$

$$= 36 \div 1 \cdot 8 \text{ less } 2 = \frac{36}{1 \cdot 8} - 2 = 20 - 2$$

$$= 18 \text{ nearly.}$$

This is slightly too much. The square root of 1 less than the square of $\frac{18}{2}$ or $\sqrt{\frac{1}{4}\left(\frac{d}{f} - 2\right)^2 - 1}$

$$= \sqrt{81 - 1} = \sqrt{80} = 8 \cdot 944;$$

therefore

$$m = \frac{1}{2}\, 18 + \sqrt{80} = 17 \cdot 944, \text{ formula (II.) note.}$$

The magnifying power at 36 inches, when it magnifies 9 times at 20 inches, is 17·944 = 18 times nearly.

On estimating Minute Magnitudes.

When very great magnifying power is employed, as 4000 or 8000, two thousandths upon the stage micrometer cannot be seen, as the field of view in general cannot embrace two consecutive lines of the micrometer. In order to overcome this difficulty I devised the following method. A very coarse scale is drawn on glass with diamond lines, or photographic lines, and is mounted in a cap or tube that fits upon the under part of the sub-stage in a fixed distance; into the upper part a good half-inch objective is inserted, and by the method already described a fine miniature of the lines is thrown upon the field of view.

Better still, I fix a parallel wire micrometer below, and throw the image of the wires upon the stage. The half inch, if the micrometer wires be 5 inches below the stage, will diminish it nearly 11 times; if $\frac{1}{4}$ be used the scale will be diminished nearly twenty times, and the wires will appear to be separated about the 20th part of the actual space, which, however, can be previously ascertained. The great superiority of this method consists in the absolute steadiness and the preservation of the light and definition. I have often found it quite hopeless to measure accurately minute bodies of difficult definition, whilst using the Jacksonian micrometer in the eye-piece, the intervening micrometer glass plates on which the divisions are drawn entirely obscuring and destroying a fine definition displayed at last, perhaps by very great pains. Further, the positive eye-piece micrometer, by the vibration it causes upon the least touch of the divided milled head, renders the distinct measurement a work of great difficulty. But by this method the micrometer lines can be reduced to any degree of fineness, and solid wires of any required diameter be substituted for the spider lines.

The advantages are these. In observing a delicate object we are frequently changing eye-pieces or objectives, altering the cover adjustment, or using water or dry lens; under these circumstances the final measurement of a delicate object, with great difficulty seen at all, requires to be done without any change whatever of the observing instrumentation. It is at that point that a delicate measurement is most desirable, when often the means fail us. But if we can apply a miniature micrometer imaged at once upon the field of view, the object is instantly obtained without disturbing the oculars or objectives.

Remark.—A covered moderator lamp, a white cloud, or diffused illumination, is necessary to define the miniature micrometer properly, and a half-inch or inch object-glass for condenser.

VIII.—*On a Method of estimating the Optical and Actual Thickness of Microscopic Covering Glass.* By G. W. Royston-Pigott, M.A., &c.

The importance of covering glass in microscopic research is daily becoming more evident: and having occasion to use a new 1–50th immersion of Powell and Lealand, the only one in existence, it became necessary to devise a means of readily measuring the thickness of the cover of any mounted object I possessed. I therefore proceeded to find data for a ready estimation.

First of all it was necessary to become possessed of an accurate gauge. Two methods are then necessary: to solve the problem. Actual thickness of various specimens should be tabulated by the gauge (I write on them with a diamond). I can strongly recommend the Swiss gauge sold by Messrs. Ehnhuus and Co., Frith Street, Soho. The instrument in my possession readily measures, with two jaws and a dial-plate and index, to the ¼th of a thousand.

I find 16 divisions make seven thousandths, and a whole circle of 100 divisions, 45.

The thinnest glass in my possession measures 2⅙ thousandths.

Having done this, the next step is to ascertain the interpretation of the divided head of the "fine screw adjustment": mine is divided into 20 parts, and by measurement the screw has 50 threads to the inch. By means of a micrometer microscope, applied to the Powell and Lealand stand, I ascertained that—

14 turns of milled head measured 65·25 divisions.
11 microscope micrometer divisions measured ·02 inch exactly.

By rule of three—

14	turns equal	.. .. ..	·1186 inch.
1	turn equals	.. .. ..	·0085* „
$\frac{1}{20}$th	of a turn equals	.. ..	·0004 „
2	divisions equal	.. ..	·0008 „
3	„ „	.. ..	·0012 „
4	„ „	.. ..	·0017 „
7	„ „	.. ..	·0029 „
14	„ „	.. ..	·0059 „
19	„ „	.. ..	·0080 „

Now, in consequence of the lever of the fine adjustment acting in a curve tangentially, the values of the divisions on the milled head fine adjustment cannot be uniform.

To ascertain these variations I examined a thick cover optically:—

Readings ..	13 divisions	.. ..	Milled head fully out zero.
	13·3 „	.. ..	Median position.
	12·5 „	.. ..	Fully home zero.
	12·5 „	.. ..	At one turn out.
	13·0 „	.. ..	At two turns out.

Conclusion; for the first six or seven turns of the milled head when fully out, the divisions may be considered as representing equal spaces in the axial line of fine adjustment.

I now selected three different covers, and very carefully measured by the Swiss gauge their actual thickness; secondly, the optical thickness; and thirdly, the full thickness, reading both by the fine adjustment divisions. Tabulated results in the three cases:—

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* ·84740 is the exact result.

	COVER I.	COVER II.	COVER III.
Actual thickness	6″ (thousandths)	4″·6	2″·2
Image reading, fine adjustment	9 divisions	6″·9	3″·3
Full thickness, „	19 „	14	7

The average reading by the table previously calculated would have been considerably at variance with these results. In my Swiss gauge the image reading is just half as much more than the thickness in thousandths or the actual thickness measured by the divisions in the fine adjustment at the rate of 3 divisions to 2 thousandths of *real* thickness.

Some interesting facts here appear: that the optical thickness measures the actual for the same kind of glass in every thickness of cover; and that the refractive index is so high in this kind of glass that the image rises considerably more than one-third of the thickness, as commonly stated loosely enough, on the assumption that the index of refraction is 1·500.

Sir John F. Herschel gives the following measures of the rays of *mean refraction* of different kinds of glass:—

Plate glass		1·500 to 1·540	For the line D in the Spectrum, or for mean rays.
Crown „		1·525 „ 1·563	
Flint „		1·576 „ 1·642	
Glass lead Flint	3 4	1·732	
Glass lead Sand	2 1	1·987	
Glass lead Flint	3 1	2·028	

I have frequently asked opticians who supplied me with the best glass covers, what was the nature of the glass, but could get no answer. I believe it is flint glass. Messrs. Chance and Co., of Birmingham, could tell us what its composition is, and give us perhaps information as to its refractive index.

NEW BOOKS, WITH SHORT NOTICES.

Manual of Human and Comparative Histology. Edited by S. Stricker, &c. Translated by Henry Power, M.B. Lond., F.R.C.S., &c. Vol. II. The New Sydenham Society. London, 1872.—The second volume of this work has made its appearance in England during the past few months, and we are promised the third volume in a comparatively short period. It is a peculiar work, having its several articles written by different authors, but for this reason it is eminently an excellent and yet it is also an unequal book. There is apparently a want of Editorial guidance, and it rather looks as though Herr Stricker allowed each individual author to write just as much or just as little as he pleased, and upon whatever plan seemed most desirable to him. The consequence of this is inequality of articles and absence of any definite scheme. For instance, some papers are decidedly too short, while others appear to occupy too long a space; some are accompanied by a copious German bibliography, while others have nothing in the form of a list of authorities. Again we must say that we consider that English work has been almost completely overlooked; of course such articles as those of Allen Thomson, Lionel Beale, Huxley, L. Clarke, and a few others are referred to, but "what are they among so many" German authorities? And, indeed, the only writer who has attempted a full list of authors is Herr Waldeyer; yet from his list of German workers and a few French and English names one would naturally be led to believe that Deutschland was essentially the country where the microscope flourished in the hands of the human anatomist. This is of course objectionable, and it is the more so from the fact that it is an evil of very old growth, and unfortunately it leads most readers to fancy that microscopical anatomy is, so to speak, a dead science in this country, which is *par excellence* the parent of the microscope. Of course Mr. Power bears no blame for this, it is solely the authors of the papers themselves, whose ignorance of English work is very much to be regretted. The Editor has done his duty, as is his custom, with patience and skill, and has rendered into terse and pleasing English what in some instances must have been horribly long-winded German phraseology. On the whole we have every reason to be well satisfied with the volume, for it brings home to us in a condensed form the various opinions of men who are not mere compilers but actual workers at the particular subjects on which they have undertaken to give us their ideas.

We cannot possibly do more than allude very briefly to some of the voluminous contents, leaving to our readers the more pleasant task of devoting themselves fully to any particular subject which may interest them. The paper on the liver, which opens the volume, and is from the pen of Professor Ewald Hering, of Vienna, is in our opinion the least valuable in the volume. This appears to us from its brevity, from its comparative novelty, and from the fact of the author's negligence (in print at least) of existing workers; for the few he has mentioned in the footnotes constitute but a tithe of those who have worked at this important subject. It seems to us that of all opinions on this subject, those of Dr. Handfield Jones, F.R.S, in his splendid memoir in the 'Phil. Trans.,' are the most worthy of belief; yet Dr. Hering does not appear to be acquainted with it. Dr. Beale's opinions too are dismissed in a word or two. This is essentially unsatisfactory.

The lungs, which are written upon by Franz Eilhard Schultze, are better done. The author has gone more carefully into his subject, and his opinions do not startle us so much as Hering's do. Still we have the fault to find with his diagrams which must be experienced by every genuine student, *viz.* that they are too picture-like. We should venture to say that not one-tenth of them are actual copies of the specimens. In this we think foreign authors are much to blame. In a drawing of Beale's, or Huxley's, or any other English worker, we are introduced to exactly the specimen which was seen beneath the microscope. But most assuredly no student who is familiar with human microscopic anatomy will take the drawings which illustrate the greater part of this book as accurate representations of the object. We think too much cannot be said against this habit, for whenever a detail not present in the object is introduced into the picture, it is almost invariably something which bears on the author's opinions. We therefore object most strongly to such figures as being anything but semi-diagramatic. We observe also in regard to this article that almost the only English author mentioned is Dr. Williams, who wrote the paper in Todd's Cyclopædia so many years since.

The section devoted to the structure of the ovary and parovarium is a very good one. Herr W. Waldeyer has given us an able *résumé* of the views of Henle, His, Klebs, Pflüger, and others. He has entered, too, pretty largely into the comparative anatomy of the subject so far as it bears upon views of the human structure, and his style is clear and to the point. Indeed, we may say that this is one of the best articles in the volume. Among other points of interest is the additional weight which the author gives to Pflüger's view with regard to the share taken by the follicular epithelium in the formation of the *corpus luteum*; he being of opinion, contrary to Kölliker, that "both elements of the follicular wall contribute to its formation." Another important feature in this paper is the very elaborate bibliography which the author appends, which, although it partakes less or more of the character before referred to, nevertheless covers more than eight pages.

Few of the other articles have very much import till we come to those upon the spinal chord by Herr J. Gerlach, and the brain of mammals by Herr Theodor Meynert. These two papers, by very eminent authorities, constitute considerably more than one-third and very nearly one-half of the entire volume. They are papers that redeem the character of the work, which would sink to comparative insignificance without them. And of these two, unquestionably that upon the brain is the best and most exact in the information it contains, and is most valuable not alone for the knowledge it sets forth, but for the excellent bibliographical list which it has appended to it. This, which covers more than five pages, is arranged in sections corresponding to the following parts: brain as a whole; cerebrum, cerebellum, cerebral ganglia; Crus Cerebri, Pons Varolii, and medulla oblongata. In the first chapter—that devoted to the brain—the lecturer on anatomy will find all that is really known of the structure of the brain, and of the origin of the cerebral nerves traced much further than they are given in our standard works on Anatomy and Physiology. He will observe too that many novel points are suggested in regard to those marvellous organs, which even yet are a puzzle to the most patient student, though infinitely less so than they have been even of late years. In regard to the illustrations, we can only award them the very high praise which they certainly merit. They have less of the Artist and more of the Anatomist about them than any which the work contains, and they have the further merits of being both numerous and novel. We think the author has done well to mention, as he does with praise, the names of his draughtsmen, Herr Dr. Carl Heitzmann, and Herr Adolf Göhre, of Vienna, whose labours most assuredly merit the greatest commendation.

The book as a whole is a very satisfactory *résumé* of the latest research as it is taught in the best schools of Germany, and it is one which we welcome accordingly in the most thorough fashion. We hope soon to have the third volume on our table, and meanwhile our best thanks are due to Mr. Power for the very lucid text which he has given us, and to the publishers—whoever they may be—who have turned out a handsome and creditable volume.

A. E.

PROGRESS OF MICROSCOPICAL SCIENCE.

Pathological Microscopic Specimens.—The 'British Medical Journal' states that a number of the above were exhibited at the meeting of the Association. Dr. J. Batty Tuke exhibited microscopic sections illustrating miliary sclerosis of the brain, colloid degeneration, fuscous degeneration of brain cells, &c. Dr. Herbert C. Mayo exhibited microscopical sections of brain structure in health and disease; and Dr. J. Thompson Dickson exhibited some sections illustrating general paralysis, explaining that the case was one in which no mental symptoms had occurred.

The Embryology of Terebratulina.—Mr. Edward Morse, who is known to be an authority on this group of animals, has just published in 'Silliman's Journal' (October, 1872), a preliminary account of the above, the full account being reserved for the Boston Society of Natural History. For several years past the author has made a special study of the Brachiopoda. The publication of the results of these investigations has been purposely delayed, till he could incontestably demonstrate the genital nature of the Cuvierian hearts, so plainly shown to be oviducts by Hancock and Huxley, and till something at least could be given of the embryology of some brachiopod. For these two matters he has visited Eastport, Maine, for the third time, and now his heretofore fruitless endeavours have met with success. He had before seen the ciliary lining of the oviducts in Lingula and Terebratulina, but he wished to see the eggs in their actual passage through the tubes. This he has now repeatedly observed in Terebratulina. The eggs were soon discharged from the sinuses in the pallial membrane, afterwards floating freely in the perivisceral cavity; the eggs were then seen gathered at the trumpet-shaped mouth of the oviduct, and have been watched as they were slowly passing through the tube, and have been caught as they were discharged at the external orifice. These eggs have then been followed in their development until they assumed the form of a deeply-annulated embryo, composed of four distinct rings, which had a marked vermian contraction upon each other. At this stage they appeared to be attaching themselves by the caudal segment. During the latter part of this examination his embryos were unfortunately lost. He had not the necessary appliances to keep the water at the frigid temperature they were accustomed to, and the increased temperature of the water led to a rapid development of Paramæcia, and other infusoria, and his embryos were ruthlessly eaten up. He has, however, nearly three hundred outlines of the embryos during their development. Next year it is hoped a complete history of their development will be made, as many things have been observed in their proper management of which he hopes to profit in his next attempt. There were also discovered prominent glands at the external openings of the oviducts in Terebratulina, which he has every reason to believe represent the testes. These glands surrounded the external orifice of the oviducts, which protruded somewhat from the anterior walls of the body, and the glands were invariably found filled with spermatozoa. From Eastport, Maine, he hurried to the St. Lawrence, with the hopes of securing some data regarding the embryology or early stages of another brachiopod found there, *Rhynchonella psittacea.* He was altogether too late for this, but had the opportunity of studying *Rhynchonella alive*, to note the ciliary action in the oviducts driving currents outward, and to establish the correctness of Owen's supposition that the arms of Rhynchonella can be protruded. A jar of specimens dredged by Dr. P. P. Carpenter, who had accompanied him from Montreal, was left standing undisturbed for twenty-four hours, when one of the specimens protruded its arms their entire length from the partially opened shells. He poured the sea water carefully out, and suddenly poured in the strongest alcohol, and the specimen is now preserved in this exerted position. In a forthcoming memoir of the Boston Society of Natural History all the details of these examinations will be given.

Some New Species of Graptolites have been described by Mr. John Hopkinson, F.R.M.S., in the 'Geological Magazine' for November, 1872. The subject may interest some of our Fellows, but the paper is impossible to abstract in any reasonable space.

Tornaria is the young condition of Balanoglossus.—Professor A. Agassiz, writing to the 'American Naturalist,' for November, states that the "development of Tornaria has at last been solved. As is well-known, Müller, Krohn, Fitz Müller, and myself have considered it a star-fish embryo. The analogy between a Brachiolaria and Tornaria seems complete, and no one questioned the position of the latter till Metznikoff in 1870 was fortunate enough to raise Tornaria to a later phase of development—to his astonishment it changed into an annelid. Of course, in view of the affinities (first suggested by Huxley) of the worms and echinoderms, it was most important that Metznikoff's observations should be repeated, and if possible the genus of annelids, of which Tornaria was the young, accurately ascertained. The annelid raised by Metznikoff was most peculiar, and, in absence of other evidence, he suggested the possibility of its being a young Balanoglossus. I have been able this summer to raise Tornaria and to obtain young annelids somewhat older than those observed by Metznikoff, tracing at the same time the development of the branchiæ as diverticula from the œsophagus, and also to find the young annelid of Tornaria a species of Balanoglossus (of which the adult is quite common at low-water mark at Newport and at Beverly, Mass.), but slightly older than those raised directly from the Tornaria stage. The details of this interesting embryology will shortly be published."

NOTES AND MEMORANDA.

How to Measure Angular Aperture.—The following plan, whose novelty is almost questionable, is given in a recent number of the 'American Naturalist':—For those students whose stands have not a graduated rotating base, Professor T. D. Biscoe recommends a plan especially applicable to the larger form of achromatic condensers which cannot be as conveniently arranged for the method advised by Dr. Carpenter. He places the combination horizontally, with a gas flame several feet distant behind it to furnish nearly parallel light, holds a card across the centre of the front lens so as to bisect the cone of light, and with a fine pencil marks the edges of the illuminated portion of the card. A common protractor is used to measure the angle of the lines thus drawn. Though chiefly useful to measure the eye-piece form of achromatic condensers (Webster's condensers, &c.) as actually used with diaphragm, &c., in position, the method may be sometimes convenient for objectives of low power. Professor Biscoe believes it can be applied to objectives as high as $\frac{1}{4}$ inch of 110° and be reliable within one or two degrees.

A New Erecting Prism.—The 'Journal of the Franklin Institute' (U.S.A.) states that Mr. Joseph Zentmayer exhibited at a meeting of the Franklin Institute a single prism which erects the image completely and in such a way that the incident and emerging rays are parallel, which, as far as we know, was never accomplished before. In connection with the microscope, as it was shown, it interfered very little with the definition, and, although the light is twice refracted and reflected, the loss of light is much less than one would expect. With the microscope, the prism is placed right above the objective, and the instrument may be used in any inclined position. A pair of such prisms might be used also for an erecting binocular microscope, of which the two bodies have the same inclination to the stage. The projection of the front is a square, that of the profile an isosceles triangle. The angles at the base of the triangle are 27°·19 for crown glass of a refracting index of 1·53, in order to obtain the greatest aperture combined with the smallest prism.

Logwood Staining of Tissues.—I wish now, says Dr. Arnold, in the 'Lens,' to describe a process that seems to do away with the two objections raised against carmine. This method I have made use of for nearly two years, and the excellent results it gives can be vouched for not only by myself, but by many students who have worked under my directions. In fact, it has taken the place of carmine in my laboratory to a considerable extent. The colouring matter of logwood (*Hæmatoxylon Campechianum*) is one of the most valuable staining materials known to me. This substance was, I believe, first used by C. F. Müller, in his investigations on the cornea. The method which is recommended by Müller can be found in the works devoted to microtechnology. I wish to give merely the manner in which the staining solution is made and used in my own laboratory. The ordinary logwood extract is finely pulverized in a mortar and about three times its bulk of alum (in powder) added; the two ingredients are well rubbed up together and mixed with a small quantity of distilled water. The *complete admixture* of the alum and hæmatoxylin is necessary, and this will require fifteen to twenty minutes' vigorous stirring. More water may now be poured on, and the solution, after filtration, should present a clear, somewhat dark-violet colour. If a dirty red is obtained, more alum must be incorporated, and the mixture again filtered. By always having an excess of both alum and hæmatoxylin in the mortar, a saturated solution can be obtained, which, after filtration, may be combined with alcohol,—one ounce of the logwood fluid with two drachms of seventy-five per cent. alcohol. I have found that a much better colour can be had by allowing the mixture, after thorough trituration, to stand for several days before filtering and adding the alcohol. Should a scum form on the surface of the liquid after it has been some time made, a few drops of alcohol and careful filtering will be all that is required. With a strong solution, such as has been described, the colouring is very rapid, requiring but a few minutes; whereas if a slower tinting be desired, the fluid may be diluted with a mixture of one part alcohol and three parts water. Whether the tissue has been previously hardened in alcohol or any of the chrome compounds, it is coloured equally well,—the nucleus of a most brilliant purple, the cell-body of a distinct neutral tint. An overstaining causes an almost perfect blackening of the nucleus, while the protoplasm becomes purple. In many instances where carmine has given but indifferent results, hæmatoxylin will be found to succeed most admirably. As to the stability of the colouring, I need only remark that specimens tinted and mounted in December, 1869, show as perfectly as when first put up.

Nitrate of Silver in the Examination of Tissues.—Col. Dr. Woodward states that a solution of one part of chemically-pure nitrate of silver in four hundred parts of distilled water (by weight) is perhaps most generally useful, though stronger solutions may be needed for special cases. The bottle should be covered with yellow paper to exclude the light. For the investigation of the serous membranes and other large epithelial surfaces, it is merely necessary to pour a little of the solution upon the piece selected for examination, or to brush the surface lightly with a camel's-hair pencil dipped in the reagent. The piece is then thoroughly washed with distilled water and exposed to the light, preferably to the direct rays of the sun. To obtain the best effects, the tissue should be taken just after death, or at least should be as fresh as possible. It should be laid in a little distilled water, in pure glycerine, or in alcohol, during its exposure to the sunlight. The result may be regarded as satisfactory if, after a few minutes' exposure, the boundaries of the cells are everywhere mapped out with delicate black lines without other visible black deposits, either on the surface or in the substance of the tissue. If the same solution is injected into the blood-vessels or the lymphatic trunks, a similar effect is produced, provided the parts are sufficiently thin and transparent to permit a prompt exposure to the light. The urinary bladder, mesentery, lungs, lymph-sacs, skin, and other parts of the frog offer excellent materials for this method. The blood-vessels, however, are apt to contract irregularly when thus injected just after death, and for the display of their endothelium it is perhaps best to employ the solution of gelatine and silver described by Dr. N. Chrzonszczewsky, of Charcow. Half an ounce of fine gelatine is dissolved in four ounces of distilled water, and to this is added a solution of one scruple of nitrate of silver in two fluid-drachms of distilled water. This fluid is to be injected warm, like any other injection-fluid containing gelatine. The syringe should be of silver, or of hard rubber with a silver nozzle. The precise quantity of gelatine must vary somewhat with its quality, the point being to use a fluid which will gelatinize handsomely when cold; the vessels are thus kept handsomely distended, and are consequently better suited for examination under the microscope.

CORRESPONDENCE.

"Using the $\frac{1}{8}$th Binocularly."

To the Editor of the 'Monthly Microscopical Journal.'

Padnal Hall, Chadwell, Essex, *Nov.* 6, 1872.

Dear Sir,—Permit me to state that there is no foundation for the assertion contained in the letter by Mr. S. Smith, on page 247 of the last 'Monthly Microscopical Journal,' that I am engaged in perfecting a Binocular Microscope that could be used with a $\frac{1}{5}$th. My instrument has always acted perfectly with that power giving a *full* field. With my own $\frac{1}{8}$th it acts also very well, simply because the object-glass is "dumpy," or, in other words, has a very short setting, so that the prism comes closer to the back lens. I believe that improvements enabling the Binocular to be used with the highest powers are quite possible. I have not, however, given any attention to the subject for some years past.

Yours sincerely,

F. H. Wenham.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *November* 6, 1872.

Charles Brooke, Esq., F.R.S., Vice-President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

Mr. Ingpen read a paper on "A Proposal for a Standard of Comparison of Magnifying Powers of Compound Microscopes."

Mr. Hogg said he did not quite understand how Mr. Ingpen would get rid of the disturbing elements in his calculations, when eye-pieces having different foci were used. The eye-pieces of no two makers were just alike.

Mr. Ingpen said the proposed use of the standard was not so much for competition as for real practical purposes. It was to enable the microscopist to place any objective that might come into his hands under the conditions in which he would ordinarily use that objective, and then to test its aperture by a standard which must, or must not, be recognized by other persons.

Mr. Brooke asked Mr. Ingpen whether there was any practical defect in the very simple mode of measuring magnifying powers which was proposed by Dr. Beale, and which consisted in placing a scale ruled for 1000th of an inch on the objective. On looking through the oblique glass plate, and placing the ruled scale at ten inches from it, then measuring how many divisions of the scale this known 1000th of an inch occupies; the one divided by the other will immediately give the magnifying power. It seemed to be a much more simple method of ascertaining the point. Were there any optical reasons why the plan was not so correct in its results as Mr. Ingpen's?

Mr. Ingpen said Dr. Beale's contrivance was very good, but there were two objections to it. The neutral tint glass was placed in such a position that it gave a different magnifying power from what would be seen, because the image then was formed outside; in his plan the image was formed in the exact conjugate eye-piece and dynamometer. If we could trust Dr. Pearson, that image does not vary with different eye-pieces. The distortion of area in the Huyghenian eye-piece was so great that no certainty could be felt in measuring according to Dr. Beale's plan.

Mr. Brooke: You simply want the distance between the two parallel lines.

Mr. Ingpen: The distance would vary greatly if those lines were only to occupy say a $\frac{1}{4}$ of an inch in one case and $\frac{1}{2}$ an inch in the other. He meant to say that $\frac{2}{1000}$ths of an inch would not be exactly double $\frac{1}{1000}$th of an inch, in consequence of the great distortion of area which takes place in the Huyghenian eye-piece. It could be very easily tested with a micrometer. Then the thing is where do you measure your ten inches from? A very slight difference in distance would make a very great deal of difference in the accuracy of the measurement; if the micrometer was not perfectly horizontal, and so on, you could hardly get satisfactory measurements except of one kind. He thought Dr. Beale's plan hardly came up to the requirements of a standard. He thought also that a standard of a moderately simple character fitted with a piece of mechanism which could be made very accurately would be of greater value than that. Mr. Ingpen then adduced other reasons why Dr. Beale's contrivance was not likely to give perfectly accurate measurements, and as perfect accuracy was indispensable he believed that the method of measuring magnifying powers advocated in his paper would enable the microscopist to attain such a desirable result.

A vote of thanks was passed to Mr. Ingpen for his communication.

Mr. Slack read a paper "On the Structure of the Valves of *Eupodiscus Argus* and *Isthmia enervis*, showing that their silicious deposit conforms to the general plan of deposition in simpler forms."

In reply to a question from Mr. Wenham, Mr. Slack said he thought the "eye spot" to be a light image at the focus of the minute lenses. The rays from the new illuminator did not show it as well as when more direct light was used.

Mr. Stewart said that his examinations of the markings of the Diatomaceæ led him to a different conclusion as to their nature. He believed they were usually depressions not elevations, but thought care must be taken to distinguish those belonging to the outer silicious lorica from others appertaining to the lining, or veil, which veil probably differed chemically and structurally from the rest of the valve.

The method adopted was one recommended by an American observer, and published in the 'Annals of Natural History' some years since. It depended on the relative indices of refraction between the diatom and the medium in which it is mounted. The silicious deposits in both animals and plants are of less index of refraction than Canada balsam. When mounted in this medium, they, if convex, will really act as a concave lens would in air, and *vice versâ*. To prove this examine a silicious and a calcareous spicule of a sponge in water and then in balsam. In water they will both appear *bright* when beyond the focus of distinct vision, and *dark* when within it; in balsam, however, the calcareous spicule will appear as before, but the silicious one will be dark when beyond the focus. In this experiment the compound eye of a beetle may be conveniently substituted for the calcareous spicule.

If now one of the larger diatoms be examined it will be found to behave in the same manner as the beetle's eye or calcareous spicule, thereby showing that if the diatom is of the same refractive power as the silicious spicule (which can be proved by other means) its markings must be depressions. The veil, however, is probably of greater index of refraction than the rest of the valve, and if this be the case the same method shows the markings in Heliopelta to be elevations.

Mr. Slack said he thought the markings were spheroidal, from an examination of a great many fractured valves, and from seeing some of the larger spheroids as lenses. He drew some analogy between the real diatom and Max-Schultze's artificial diatoms; and thought that the principle of chemical precipitation was similar in both. He was very much inclined to think from the examination of a great number of objects of the class in question that all were constructed on the same type. The vegetable matter of the diatom acts chemically upon the silex in the water, which is the condition of colloid silica, and the deposition always takes place in the spheroidal form.

Mr. Brooke would like to know how it happened, if the structure of these objects were really hollows and not bosses, that the line of fracture runs between the dots, and not through them. He had fractured a great many diatoms under the microscope, and had never seen a single instance in which the line of fracture did not run equidistant between two consecutive dots, which would hardly be the case if they were depressions and not elevations.

A vote of thanks was then passed to Mr. Slack.

Mr. B. T. Lowne read a paper "On the Nervous System of Insects and Crustaceans compared with that of Annuloida and Vertebrata."

Mr. Ingpen called the attention of the Fellows to a marvellous photograph of a solar spectrum made by Mr. Browning from a lithograph of Fraünhofer, which he thought was likely to prove of great value in spectrum analysis.

Mr. F. Fitch exhibited a specimen of Mr. Dalton's curiously beautiful slides of butterfly scales, arranged as a bouquet of flowers with insects.

Donations to the Library and Cabinet from October 2nd to November 6th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Journal of the Society of Arts	*Society.*
Journal of the London Institution, No. 17	*Institution.*
Journal of the Linnean Society, No. 55	*Society.*
On Callograptus radicans, *a New Graptolite.* By John Hopkinson, F.G.S.	*Author.*
Popular Science Review, No. 45	*Editor.*
Bulletin de la Société Botanique de France	*Society.*

The following gentlemen were elected Fellows of the Society, *viz.*:—

Arthur Angell, Jun., Esq.
John Thomas Atkinson, Esq.
Joseph F. Gibson, Esq.
Charles Thomas Hudson, LL.D.
Robert Marchant, Esq.
John Matthews, M.D., &c.

WALTER W. REEVES,
Assist.-Secretary.

ERRATUM.—On p. 250 of the November number, line 7 from the top, for *larvæ* read *cilia.*

OLDHAM MICROSCOPICAL SOCIETY.

The members of the Oldham Microscopical Society held their second conversazione for the season in the club-room of the Oldham Lyceum, on the 18th of October, when, though the night was stormy, a very respectable company assembled. On entering the room we were struck by the fine display of first-class binocular and other microscopes, under which were displayed the various tissues and deposits indicative of disease; and on the walls numerous well-executed enlarged drawings were hung, intended to illustrate the subjects dealt with in what may be termed the central attraction of the evening—*viz.* the paper by Dr. George Thomson "On the Use of the Microscope in Disease." Among the subjects illustrated we observed under the microscopes several specimens which displayed the peculiar forms of cell which distinguish cancerous growths, with their fibrous framework and distinct nuclei; also pus cells, treated with acetic acid; sarcini, or fungoid growths, found in the vomit under certain conditions, &c., &c. The lecturer dealt with a subject which involved most important issues, and is necessarily of a most extensive character, the difficulty being to present to the non-professional mind sufficient to enable it to intelligently comprehend the subject, at least in outline, and yet not burden it with undesirable details. This happy medium, we think, was most successfully realized, and the attention manifested throughout is a proof that such a subject is capable, when properly treated, of being made interesting to the non-medical public as well as to the profession.

Some time having been spent in viewing the objects under the microscopes, and in easy chit-chat upon them, the chair was taken by the President, Dr. A. Thom Thomson, who briefly introduced the lecturer, who said that, while remembering that the microscopical study of disease was almost entirely confined to the profession to which he belonged, he thought such a paper as he was now to read, which aimed simply at giving a sketch of what was interesting in the use to which a busy medical man put his microscope, might prove acceptable to the non-professional members of the Society. The introduction of the microscope, he need hardly say, heralded a new era in medical science. The microscopical examination of tissues in health and disease upset many a pet theory, and cleared up much that was before all obscurity and doubt. Although not entirely to be relied upon, it was undoubtedly a valuable aid in the detection of disease. Suppose, for instance, he was called to a patient who had vomited blood from the stomach, and he had no certain means of determining what was the condition of that organ. Well, he would put a little of the blood under his microscope, and might find that it contained certain cells which characterize cancerous or other diseases, and this discovery would influence the treatment of the case. Before commencing the subject of his paper proper, he would give a brief description of the various structural elements of which the body was composed, namely, granules, cells, fibres, membranes, &c. Passing by the granular matter, he proceeded to describe the various cells of which the tissues are built up. Pus, or matter taken from abscesses, he described as composed of circular cells, somewhat larger than the blood corpuscles, and granular in appearance, and stated that, when treated with acetic acid, several nuclear bodies were visible in their centres. These cells are frequently to be met in the examination of diseased tissues and secretions. When viewed under high powers they display peculiar movements, and have the power to throw out protusions, which rapidly separate themselves, and develop into new cells. Pus cells he viewed as forms of growing matter degraded from its proper use, namely, that of forming tissues, but which is increased in vitality, and, as a consequence, multiplies itself more rapidly. Blood contained a large number of red, and a smaller proportion of white corpuscles, the latter identical in appearance with the pus corpuscle. In health the proportion is about one white to fifty red, while in some diseases the white become much more numerous; in extreme cases being nearly as prevalent as the red. In cholera, and in some kidney diseases, the red corpuscles have a tendency to adhere to each other. The character of the spit, when microscopically examined, frequently enables the practitioner to determine the character of the disease from which a patient is suffering. Healthy spit is often very much charged with extraneous matter; such, for instance, as silk and cotton fibres, from the articles of clothing, feathers, starch, particles of food, dust, &c. In all spit they found mucus, which consists of a viscid mass, containing cells of various shapes and sizes.

Besides, they had special appearances in some diseases; thus, in inflammation of the lungs they had minute oil globules, numerous blood corpuscles, and granular spherical cells. The yellow spit of bronchitis abounds in pus cells and epithelium in all stages of growth. In the spit of consumption they also found pus corpuscles or globules, and peculiar bodies called tubercle corpuscles, of an oval form, but without nucleus; also, at an early stage of the disease, fragments of lung tissue, which were invariable evidence of the disease. Having alluded to the advantages to be gained from a microscopical examination of vomit, Dr. Thomson proceeded to speak of the most important part of his subject—the examination of diseased growths and tumours. This part of the paper we give in full, as follows:—"It becomes often of the utmost importance to determine the nature of one of these growths after removal by the surgeon's knife; and it often rests with the microscopist alone to settle the question as to whether a return of the disease is likely. Even before removal it is sometimes possible, by detaching a minute portion of the tumour, to subject it to the microscope, and to decide as to the probability of an operation effecting a permanent cure. The increase to our knowledge on this subject alone, since the microscope was brought to bear on it, has been simply invaluable; and I have not a doubt in my mind but that we are on the eve of discoveries still more important, which may be the means of averting, in a great measure, some of the most dreadful diseases that scourge mankind. There are two grand classes of tumours. In the one you have the tumour produced by irregular growth of some tissue of the body, and consisting simply of the elements of that tissue. Such are fatty, and bony growth, and many others, of which you have a familiar example in warts, which are simply an irregular growth of the skin, and are composed of the same elements. These are simply deformities, and fraught with no danger to life, unless under extraordinary circumstances. The other class, however, is very different. Here we have structures not comparable to any natural tissue of the body. Such are all kinds of cancerous growths, to which I shall come presently. Now, it is considered a fact beyond dispute that every tissue in the body results from changes that have taken place on germinal or growing matter. The tissue is, in fact, formed matter. Also, that this growing matter was derived from other growing matter which existed before it, and which may even have had different properties from it. Now, in every healthy tissue you will find small collections of growing matter, from which new tissue is formed from time to time, to replace what is wasting away during the performance of its function, or when a portion has been destroyed by disease. And the rate of formation of this new tissue is in proportion to the destruction of the old tissue. Now, suppose that one of these germinal masses should become displaced by an accident, such as a blow, or from some other cause, should become irregularly supplied with nourishing material from the blood, and, in consequence, sprout into active growth when not required, the result would be a shapeless lump of tissue, in fact, a tumour. Now, this tumour having no office to perform, and hence no waste to undergo, continues to grow just as a healthy tissue would do, there being no waste, however, to counterbalance the growth. Thus results a tumour of the first class. Now, as we find masses of germinal matter in health giving rise to other masses which have different properties from the first, it will not surprise us if we find that an irregular supply of nutriment, or a too rapid growth of some mass of germinal matter, should lead to the production of matter whose power to form tissue is degraded. And, as in the case of pus, while this degraded matter is never able to give rise to matter of equal value with its ancestor, yet it grows and multiplies with increased rapidity, and lives under conditions in which its ancestor would have perished. In fact, the less the power of forming tissue, the greater the vitality or power of increase of the germinal matter. These masses, then, insinuating themselves in the meshes of the original healthy tissue, increase with such enormous rapidity at its expense that they lead to its destruction. They become more malignant as they increase, and may even pass into the blood, travel long distances, and set up a fresh formation at some other part of the body. Thus we have produced tumours of the second class. As to the first class of tumours, it is sufficient for my purpose to show drawings of one or two. They have the same character very nearly as the tissues from which they spring, be they bony, fatty, cartilaginous, or simply fibrous. There are many others, not of much interest microscopically. As to the second class, the fact is there is an immense variety; the variety, however, is chiefly in degree. We have every shade of difference between a simple fibrous tumour and a typical cancer. Cancer proper consists of a fibrous framework, arranged to form interspaces which contain peculiar cells, suspended in a viscid fluid, with much granular matter, which is partly germinal and partly fatty. No one of these appearances, taken by itself, can be considered characteristic of cancer; it is only when we view them collectively that we can determine their cancerous nature with absolute certainty. Usually it is easy to decide, but there occur, every now and then, cases which puzzle the most experienced microscopist, so many and so fine are the gradations from simple tumours to those of a cancerous nature. There are several varieties of true cancer, which depend on the relative proportion of the juice, cell, and fibre elements. The fibrous parts are, as you see, just like fibres in healthy tissues. The cells vary much in size and form, from round to the most irregular shapes possible. They generally contain a large mass of growing matter, which is often to be seen divided into two or more separate masses within the cell. These cells rarely adhere to each other or to the fibrous framework. There is a peculiar form of cancer, however, called epithelial cancer, generally connected with the skin, especially that of the lips. The cells here vary less in size and shape, and are usually adherent both to each other and to the fibrous framework. They are often, in fact, arranged in regular layers in the interspaces. In the other forms this is never the case. Neither do the cells resemble that of true cancer in the subdivision of the germinal matter forming cells within cells. This form of cancer is not nearly so malignant as true cancer, and generally runs a much longer course before causing death."

Various other subjects were alluded to, and at the close a very cordial vote of thanks was passed to Dr. G. Thomson, on the motion of Dr. Platt, seconded by Mr. Pullinger. A very instructive evening was spent by the company.

LIVERPOOL MICROSCOPICAL SOCIETY.

The eighth meeting of the present session of this Society was held last month, in the Royal Institution, Colquitt Street, the Rev. William Banister presiding. An adjourned discussion took place on Mr. Newton's paper "On Spontaneous Generation," and on Dr. Bastian's work, "The Beginning of Life."

In the absence of the President, from indisposition,

The Rev. W. Banister opened the discussion. He said,—The subject for our consideration this evening is one of the most interesting in the whole range of microscopic study. There is a mystery about life—where it is, how it begins, and where it ends (I mean materially, not theologically), which fascinates all students of nature. One objection to Dr. Bastian's views, perhaps, is that they tend to solve this mystery, and to take a good deal of sentimentality out of our notions about it. I do not, however, think there is any probability of our solving the mystery. Even if all Dr. Bastian states is true, it only proves that animal and vegetable life, in its lowest form, is substantially the same, but it does not tell us where the vitality that animates an aggregation of bacteria comes from; still less does it account for the possession of so wonderful a property by each individual bacterium. There are two ways of proving, as far as possible, whether living matter can arise *de novo* without the previous existence of "germs." The one is to submit the infusions to so great a temperature that all vitality must be destroyed; the other is to examine the air minutely to see whether the "germs" exist in it in sufficient abundance or not. Both these tests require delicate and costly apparatus, and great skill, experience, and patience in the use of it. Those who do not possess these requisites must be content to accept the statements of those who do, provided they have reason to believe that the experiments have been carefully conducted and conscientiously interpreted. This may be ascertained by a comparison of the results arrived at by different experimenters of repute, and by a personal examination of such minor details as may be observed by students who possess moderately good instruments. If the facts observed, as far as they go, bear out the statements made, we are justified in believing that such statements as we are not able personally to verify are honestly in accordance with apparent fact. I say apparent, advisedly. The higher the powers used the more danger there is of error in the interpretation of what we seem to see. This is all that I have attempted to do; and I am bound to say that so far as I have observed, all the initial developmental changes which Dr. Bastian describes do take place. Dr. Bastian's theory is that in a fermentable solution there is a change from a diffused imperceptible state to a concentrated perceptible state, the form usually first assumed being that of bacteria—vibriones and torulæ—the first being the simplest. These bacteria, or specks of living matter, as they increase in size, acquire a structure, develop organs, and become animals, or vegetables of more or less complicated nature. These aggregate and form the "proligerous pellicle" of Pouchet, which is composed of a dense mass of bacteria, generally end up, with some vibriones and torulæ imbedded in a pellucid gelatinous material. As two amœba coalesce and form one, so these bacteria fuse and form organisms. It seems to me that the truth or error of these statements may be verified by most members of this Society, and that we shall not perform our duty as members of it, or take any respectable rank among kindred societies, if we confine ourselves to the exhibition of petty things, and neglect the highest objects of microscopic study.

Mr. G. F. Chantrell, hon. secretary, said he considered Dr. Bastian's book one of the most valuable contributions to biology that has yet been made. It is a book that should be in the hands of every microscopist. It teems with strange and marvellous facts in infusorial life. Dr. Bastian has resuscitated the observation of several almost forgotten writers, and has verified some of their facts, which were little noticed at the time, and to which he has added a large number of his own observations. He agreed with Dr. Bastian that we are but on the threshold of our acquaintance with the marvellous heterogenetic transformations, the discovery of which already affords material for revolutionizing the old foundations of botanical and zoological science;—that the path opened must be followed by faithful and competent observers, true students who are willing zealously to watch and wait through eager hours whilst nature unfolds her secret processes, unblinded by any existing theories, and content to regard them as useful and modifiable aids to further progress. It was in the spirit of such a student he offered the following observations. He described the remarkable seed of the collomia plant, and the results he had obtained by keeping a single seed with a drop of distilled water in a live box. Almost immediately he had observed life, using a power of 5 to 700 diameters, and in course of a few days the glass became crowded. A single seed was kept under observation for above a month (the water evaporated being supplied occasionally), and during the whole time paramecia abounded. After a while a remarkable change took place; the paramecia had developed into six or seven different forms, of which he exhibited drawings. These died off, leaving a ring of dead substance, out of which soon appeared spots of granular matter, and these afterwards developed into perfect amœba. Dr. Bastian states in the preface to his book, that the spontaneous generation theory rests upon the possibility of the *de novo* origin of bacteria, and that evidence of the most convincing character shows that bacteria are killed by a temperature of 140° Fahrenheit. Mr. Chantrell quoted, in opposition to this, Dr. Crace-Calvert's experiments, showing that the greater portion of microscopic life was destroyed by a temperature of 200° F., but that black vibrios were not killed by 300°, and then instanced his own observations upon boiled vegetables. He had frequently taken small portions of cooked vegetables which had been boiled for not less than half an hour, and invariably (with a power of 760 diameters) found life, and, in twenty-four hours afterwards, bacteria in abundance. From cooked vegetable marrow, on the second day, he had found ciliated infusoria, a monad, which appeared to have come from the tissues. On the fifth day he observed a fungoid growth set up, no doubt induced by the want of air. After describing a number of other observations, most of which were illustrated by drawings, he said that in all his examinations of cooked vegetable marrow, turnip, carrot, celery, potato, &c., all alike show these minute organisms. Again, in examining cooked hare, he had found these minute trembling specks, similar in every respect to those seen in the vegetable, and alike accompanied by bacteria. How strange that the animal and vegetable kingdom should assimilate so closely! Mr. Chantrell then remarked on the singular property that bacteria have of uniting and forming filaments. In the mature plant, or animal, it is ever on the move, coiling itself up and uncoiling; the movement of the spirillum is as yet a mystery. By an attentive study of boiled vegetables in glass troughs much may be learned. In the last few weeks he had been able to confirm several of Dr. Bastian's observations as well as others quoted by him, and he trusted that the members would follow up this study of home microscopy during the long evenings of winter, and they would learn to appreciate Dr. Bastian's valuable contribution. In the last few weeks he had learnt more of infusorial life by the aid of this book than in months of previous study. He said he was content to go working steadily on, neither declaring in favour of one hypothesis or the other. He feared the conclusions were too hasty; there is sadly too much tendency to set up theories and stick to them. We want a class of observers who will watch these extraordinary changes with the same fidelity that the telegraph clerks watch every movement of the magnetic needle of the Atlantic cable at Valentia, and, as the Christmas story has it, be, indeed, "slaves of the lamp." We want slaves of the microscope, who will faithfully record every change in these remarkable evolutions of infusorial life.

A discussion followed, in which Mr. Newton, Dr. Inman, and Dr. Drysdale took part; and on the motion of Mr. R. Hamilton, F.R.C.S., the discussion was adjourned to the December meeting.

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